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ARTIFICIAL VENTRAL ANUS IN A PONY.

BY W. E. LITT, M.R.C.V.S., SHREWSBURY.

On Aug. 18th last I was requested to see a brown horse pony, about twelve hands, and five years old, the property of a clergyman in this neighbourhood. The pony had on the previous day, whilst being driven, become frightened by an accident to the reins, and run away, ending by jumping at a low wicket-gate, over which he managed partially to get, but becoming suspended, with the whole weight of his hinder parts upon the abdomen, this being fixed upon the points of the pales of the gate, which was fortunately of wood. When I saw the pony there was a very considerable ventral hernia in the right iliac region, and another in a corresponding position on the left side. On manipulation it was quite clear that in that of the right side the whole of the muscular and tendinous parietes were divided, and the intestine retained merely by the skin. On the left side there was only a partial division of the abdominal wall. I decided to use a compress, and accordingly fitted one with three straps and buckles across the back, which could be gradually tightened or otherwise as required. I ordered the pony to be fastened up by the head in a stall, and sparingly feed with bran mashes and hay; but I prescribed no medicine, as there was no constitutional disturbance whatever. On Sept. 3rd I again saw my patient, and on removing the compress was surprised to find that the pad applied to the hernia on the right side was saturated with moisture, and on bringing the pony out into the light a fistulous opening in about the centre of the tumour was apparent. There could be no mistake about the nature of the discharge, which consisted of the liquid and pulpy contents of the intestine, which ran out in jets as the pony walked along, the opening, which was of about the calibre of an ordinary pencil, being every now

and then partially and momentarily plugged by a piece of more or less solid ingesta.

I should say that by this time all effusion had subsided, and the opening into the abdominal cavity on both sides had become considerably reduced, that on the left side being hardly noticeable. On the right side, however, some two inches from the fistulous opening, there was evidence of the pointing of a small abscess, which I opened, and evacuated about two ounces of pus. Not being prepared to operate at the moment, and the patient being to all appearance quite undisturbed by this artificial anus established in his flank, I decided to wait a few days before taking any steps to close the orifice. I at once ordered the discontinuance of the compress, and the injured part not to be interfered with in any way. On my next visit a few days later, I was surprised, and need hardly say much pleased, to find that the opening was completely closed, and with the exception of a little fulness under the flank, which is not perceptible unless you stoop down to look for it, there is nothing to be seen. I have been led to record this case in the columns of your Journal owing to its extreme rarity, as far as my own experience is concerned. At the same time, I have been unable to discover that any similar case has been recorded, with the exception of one quoted by Professor Williams in his "Principles and Practice of Veterinary Surgery," as having occurred in 1837, and this case also appears to have closed without any surgical interference.

EXPERIMENTS ON THE ESSENTIAL DIET FOR HORSES, AND THE DIGESTIBILITY OF HAY AND OATS.

BY FRED SMITH, V.S., HYGIENE LABORATORY, ARMY VETERINARY SCHOOL, ALDERSHOT.

In a previous communication, I considered, from Continental observations, what the essential diet of a horse should be for twenty-four hours. From experiment made in the laboratory of this school, my results do not agree with those of Boussingault; the practical outcome of the experiments are, however, in no wise affected by the differences between my observations and those recorded by M. Boussingault.

The object I had in view was to determine the least amount of food which adult horses, *doing no work*, could live on without loss of body weight. I will distinguish the horses experimented on as A, B, C, and D. The experiment lasted twenty-two days; the horses were weighed once every twenty-four hours, as near as possible under identical conditions; the amount of fæces excreted in the twenty-four hours was also carefully ascertained; the amount

of urine passed was only obtained for one horse, owing to the great difficulties attending the collection.

The hay they were fed on of necessity varied in quality and chemical composition; but by taking a mean analysis of hay we can arrive at fairly correct results.

From the 28th August to the 10th September, the daily diet consisted of 12 lbs. hay, and from the 11th to the 18th September, 2 lbs. oats in addition. For the first day or two there is a slight fall in weight in all horses except D; no important decrease in weight occurred with this horse for seven days; on the sixth day of the experiment all but horse D had an increase in body weight; from the sixth to the fourteenth day the weights fluctuated slightly, there being in all horses a slight but decided tendency to loss of body weight.

On the fourteenth day each horse received 12 lbs. hay and 2 lbs. oats daily. A striking feature in regard to this is the slight increase in body weight which it produced.

The reason why the body weight fell slightly throughout the whole experiment, and why the 2 lbs. of corn daily did not add more to the body weight, is explained by the fact that these horses stood the whole time of the experiment; had they taken their night's rest regularly, no loss, I think, would have occurred. The reason why they did not rest is because I gave them no bedding, in case they ate the straw and thus vitiated the results of the experiment; I had no moss litter or other substitute to give them.

The average daily amount of fæces passed on 12 lbs. of hay was:—A, 24 lbs. 2 oz.; B, 23 lbs. 7 oz.; C, 21 lbs. 3 oz.; D, 23 lbs. 4 oz. Containing dry matter:—A, 6 lbs.; B, $5\frac{3}{4}$ lbs.; C, $5\frac{1}{4}$ lbs.; D, $5\frac{3}{4}$ lbs.

The amount, therefore, digested out of the 12 lbs. of hay (deducting $1\frac{1}{2}$ lbs. for moisture naturally in it) was:—A, $4\frac{1}{2}$ lbs.; B, $4\frac{3}{4}$ lbs.; C, $5\frac{1}{4}$ lbs.; D, $4\frac{3}{4}$ lbs.

The percentage digested of food supplied was, therefore:—A, 42·8 per cent.; B, 45·2 per cent.; C, 50 per cent.; D, 45·2 per cent.

The average daily amount of fæces passed on 12 lbs. hay and 2 lbs. oats was:—A, 18 lbs. 3 oz.; B, 20 lbs. 11 oz.; C, 22 lbs. 12 oz.; D, 21 lbs. 8 oz. Containing dry matter:—A, 4 lbs. 8 oz.; B, 5 lbs. 3 oz.; C, 5 lbs. 11 oz.; D, 5 lbs. 6 oz.

The amount, therefore, digested from the 12 lbs hay and 2 lbs. oats, after deducting $1\frac{3}{4}$ lbs. for their moisture, was:—A, 7 lbs. 12 oz.; B, 7 lbs. 1 oz.; C, 6 lbs. 9 oz.; D, 6 lbs. 14 oz.

The percentage digested of food supplied was, therefore:—A, 62·5 per cent.; B, 56·4 per cent.; C, 52·4 per cent.; D, 56·2 per cent.

In the first half of the experiment, the average amount of dry matter in the fæces from the four horses, out of 12 lbs. hay, was:—5·69 lbs., and the amount digested 4·81 lbs. This 4·81 lbs. was probably made up as follows:—

Albuminoids	·656	= 1575	foot tons of energy.
Fats	·100	= 604	,, ,,
Carbo Hydrates ...	2·374	= 8174	,, ,,
Cellulose	1·330		
Salts	·350		
	4·810	= 10358	foot tons of energy.

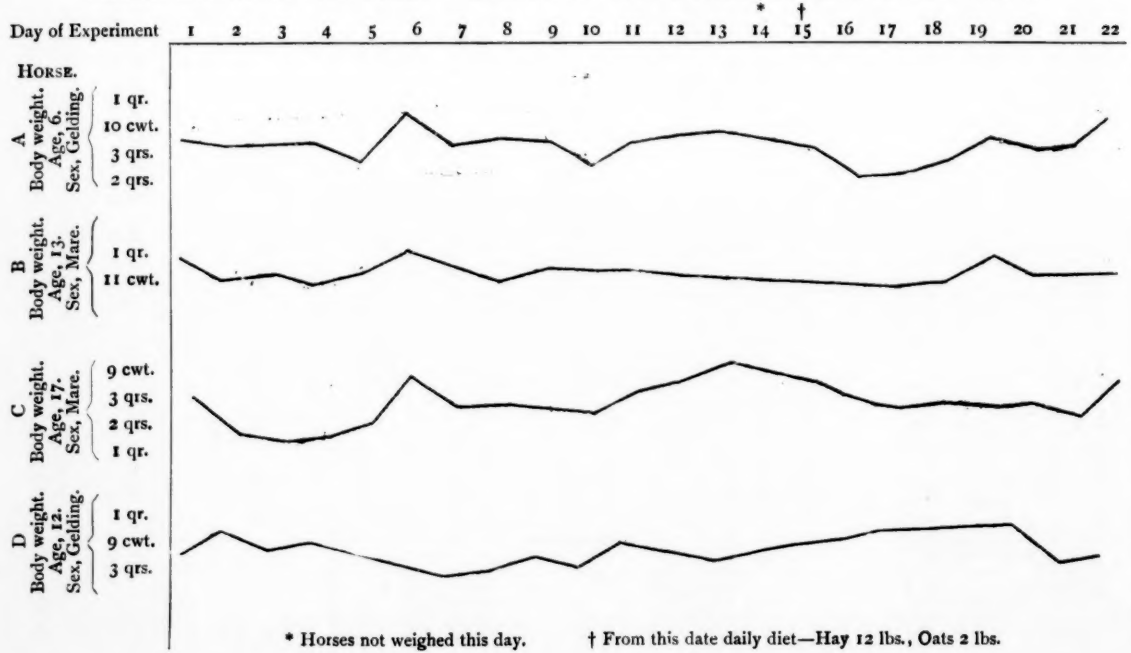
The potential energy appears very small; the portion contributed by the albuminoids I am sure is correct, for I found that horse A was excreting 1·6835 oz. of nitrogen in twenty-four hours; the carbon portion I am not sure of, for the reason that the only means of checking it, by determining the CO_2 given off from the lungs, failed me through faulty apparatus.

When we come to consider the second half of the experiment, we are struck by the fact that the increase in body weight is not more marked. Selecting the sixteenth day in the chart as the period when increase in body weight should show itself, we find there is a very slight rise in all horses but C. This mare was a delicate feeder, was tired of standing, and digested less than any of the others; these are the only reasons I can adduce why she should not have shown an increase instead of a slight decrease in weight.

Respecting the weights of the horses, there are one or two things to be said why weights vary. A good drink of water will make a horse weigh 20 lbs. heavier; the loss of a "dropping" will make his weight 5 or 6 lbs. lighter. I took every care to avoid fallacies arising from this cause; still I observe that in one or two places horses are shown to have gained 112 lbs., or to have lost the same amount in twenty-four hours—an obvious absurdity. The scales *may* have shown these quantities, but the increase or decrease is not in flesh, but is probably in water, or fæces, or both. A few pounds one way or the other in twenty-four hours cannot be avoided, and may be entirely disregarded where an experiment is being carried on long enough to eliminate error.

The most interesting part of the second experiment in feeding on 12 lbs. hay and 2 lbs. oats, is the larger amount of food substance digested by the horses. The mean percentage of food digested when receiving 12 lbs. hay was 45·8 per cent., whilst it rose to 56·875 per cent. when feeding on 12 lbs. hay and 2 lbs. oats. Thus more than 10 per cent. increase in digestion is shown to have occurred, which is a most interesting and, to me, unexpected result.

CHART SHOWING THE DAILY WEIGHTS OF THE HORSES EXPERIMENTED UPON.



* Horses not weighed this day.

† From this date daily diet—**Hay 12 lbs., Oats 2 lbs.**

Apart from other reasons, the addition of corn to hay ration is economical, *as the addition of the oats raises the nutritive value of the whole diet, and the horse abstracts more nourishment from it.*

The practical lessons to be learned from these experiments are :—The vital processes of a horse's body require for their support the digestible nutriment found in 12 lbs. medium hay. The addition of oats to hay increases the digestibility of the latter. On the hay ration is performed the internal work of the body, on the corn ration is performed the external work.

NOTE ON SALICYLATE OF IRON.

BY JAMES BRODIE GRESSWELL, M.R.C.V.S., LOUTH.

SALICYLATE of iron, a valuable remedy for diarrhœa in calves, can be exhibited, as shown by Dr. Braithwaite, by mixing together sulphate of iron and salicylate of sodium in water. His prescription for obtaining this compound is :—

℞	Sulphate of iron	1 part (or 20 grains).
	Salicylate of sodium	1 part (or 20 grains).
	Glycerine	9 fluid parts (or 3 drachms).
	Water to	72 parts (or 21 drachms).

Of this mixture this physician gives a teaspoonful every hour for diarrhœa in children, and he continues the administration until the stools become darkened. The bowels he keeps open by an occasional dose of castor oil. Of salicylate of iron, the dose for a calf varies with the age and depends upon the frequency of administration. It is not convenient to give it more than twice or three times daily.

SKIN DISEASE DUE TO FILARIA.

BY RICHARD W. BURKE, V.S., A.V.D., STATION VETERINARY HOSPITAL, CAWNPORE.

IN horses we find, under the names of Dermatitis Pruriginosa (Rivolta) and Dermatitis Granulosa (Laulanié), a peculiar, knotty formation of the skin, which is thrown up by round worms. Ercolani noticed this disease for the first time in 1861. Laulanié (*Revue Vétérinaire*, 1884, p. 166) has described it as knots growing in the subcutaneous connective tissue, and containing within them

the peculiar coiled-up worms, which cause their growth. The skin itself becomes thicker than healthy skin. The knots are never adherent to the skin, are movable under the fingers, and the worms within them may be readily extracted, like the kernel of a nut. The disease-process begins by a slight infiltration of the cutis and subcutis. Contamine and Drouilly (*Recueil de Med. Veter.*, 1878) found hæmorrhagic knots in the skin, caused by these worms. Rivolta (*Il Med. Veter.*, 1868, p. 300) holds that removal of these worms is essential to the cure of this disease, and thinks that the so-called "tubercular granulations" of older authors were doubtless of this nature.

Siedamgrotzky (*Sachs. Jahresber*, 1883, p. 19) observed this disease on the legs and hind-quarters of a dog, in which each knot contained from one to three coiled-up worms. The tail of the worm was of a peculiar spiral form, differing in this respect from the worms seen in the horse-disease. There was much pruritis present, from the peculiar spiral formation of the dog-worm. A cure was effected generally in three weeks. Rivolta (*La Clinica Veterinaria*, 1885, No. 1) describes it under the name of the filarian skin disease of the dog.

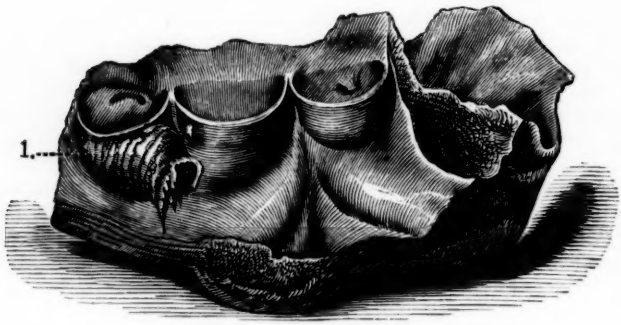
Semmer (*Oestr. Vierteljahrsschr.*, 1871, p. 179) has observed an instance of a conglomerate growth on the skin of the gluteus in a horse, which contained numbers of these filaria situated between the epidermis and cutis vera.

AORTIC-VALVE DISEASE.

BY K. I. URQUHART, M.R.C.V.S., ASSISTANT TO MR. A. H. SANTY, F.R.C.V.S., NORWICH.

HEART-DISEASE in our patients is not uncommon, but is not often diagnosed, owing to the disadvantages with which we have to contend, compared to the human surgeon, who relatively has a greater expanse of chest, thinner thoracic walls, and no thick covering of hair to apply his ear to, either directly or otherwise. Therefore I venture to offer a few remarks on a case which recently came under my notice. The subject was a bay cart-mare in good condition, the property of a contractor, to which I was called on the evening of the 24th of last month. Upon examination, I found respiration greatly hurried and laboured, pulse beating 108 per minute, jerky in character—"Corrigans," and intermittent. The normal diastolic sound was replaced by a very loud *bruit*. From the general symptoms, and on learning from

the carter that she was suddenly taken ill whilst at work, I diagnosed my case to be rupture of the aortic semilunar valve. Owing to the interference with the function of this valve, there was a damming of blood in the lungs, which made the vesicular sounds dull; the conjunctiva was pale in colour, and the internal temperature rose on the third day to 103° F. As time went on, the pericardial sac became filled with water, and a little effusion took place in the thoracic cavity. On the 1st of October, the day before death, the heart sounds were audible quite one yard distant from the chest. I need hardly state that the patient never attempted recubation during her illness. The autopsy confirmed my diag-



1. RUPTURED SEGMENT OF THE AORTIC VALVE.

nosis. The thorax contained a little water, the pericardium being full; lungs voluminous, containing a great quantity of dark-coloured blood; the heart was a little hypertrophied and flabby; endocardium of a dark hue. All the valves appeared intact, except the aortic sigmoid, which had one segment ruptured at the base, the aperture being sufficiently large to admit the forefinger, as will be seen by the accompanying woodcut. The arch of the aorta was also the seat of structural change.

GLOSSANTHRAX AND ANTHRACOID ANGINA: ITS SYMPTOMS.

BY J. B. GRESSWELL.

ITS NATURE AND PATHOLOGY, WITH REMARKS ON THE GERM PROBABLY CAUSING IT.

BY ALBERT AND J. B. GRESSWELL.

In the VETERINARY JOURNAL of February, 1885, I described a case of Glossanthrax and Anthracoid Angina in a two-year-old bay cart-filly, to which I was called on December 14, 1884. Since that time I have only seen one other case of this dreadful disease. In both, the symptoms were very similar, but it is only in the latter case that I made a special pathological examination, with the view, if possible, of throwing some light on the nature of this terribly malignant malady. Dr. Albert Gresswell kindly conducted the microscopical investigation. It is noteworthy, in the first place, as I pointed out in the number of the VETERINARY JOURNAL above mentioned, that whereas cases of Anthrax proper, or Anthrax Fever, affecting horses, are not at all uncommon in North and Mid Lincolnshire, cases of Glossanthrax are decidedly very rarely met with in the equine species.

Indeed, until recently, I should think that Anthrax was more common in Lincolnshire than in many parts; but, owing to the fact that now so many of the fields on which Anthrax was endemic have been by our orders thoroughly drained, it is much more rarely met with in this district than it was several years ago.

The subject of the first case of Glossanthrax was in good condition, and had only recently been brought up from grass. The sides of the cheeks and the tongue were much swollen, and were of a dark purplish-brown hue. The tongue was hanging from the mouth, and protruded about six or seven inches. There was a copious eruption of phlyctenules on the sides and dorsum of the tongue, and also on the buccal membrane of the lips and mouth. Thick, yellow-coloured serous fluid was discharged in abundance from the mouth and nose, and the breath was fœtid. The throat was swollen, hot and painful, and deglutition was quite impossible. The pulse, which was very feeble, numbered eighty-beats per minute, and the respiration was hurried and difficult. The temperature reached 105° F. On the following morning, the symptoms were still more exaggerated. The head and neck were immensely swollen; the tongue, still hanging pendulously, almost completely filled up the cavity of the mouth. The buccal membrane was

croded in patches, as also was the dorsum of the tongue, and yellow serum tinged with blood oozed from the mouth. The pulse was fluttering and almost imperceptible, the legs and ears were cold, the nostrils were dilated, the breath was very fœtid, the respiration was hurried and very difficult, and the animal seemed about to succumb. The swelling in the region of the head and face more especially affected the cheeks, the body and base of the tongue, and the tissues of the submaxillary, parotid, and laryngeal regions. As there was no prospect of recovery, the animal was shot.

The subject of the second case, to which I was called on October the 14th ult., at 8 a.m., was an aged brown cart-horse, belonging to a gentleman-farmer who resides on the wold-land at a distance of about eight miles from Louth.

The animal had quite recently been brought up from grass, and was noticed on the 13th to be dull and feeble, refusing all food, and discharging mucus from the nose and saliva from the mouth.

On the morning of the 14th I found the horse to have refused all food. He was standing in a corner of the loose box champing with his teeth, and discharging a large quantity of frothy saliva from the mouth. After a careful examination, I found his pulse to be feeble, and in number ninety-six per minute. The temperature was 104·8°, and the respirations were twenty-seven per minute. Examination of the mouth showed the tongue and buccal mucous membrane to be very much swollen and hot. The under surface of the tongue was denuded of epithelium in patches. The parotid and submaxillary glands were considerably swollen, and the lining of the nose was highly injected, as also was the conjunctival membrane. The discharge from the nose and mouth varied greatly in amount.

The horse was treated with antipyretic and antiseptic remedies, and all solid food was taken away, gruel only being allowed. He was caused to breathe medicated water-vapour. The throat was well fomented, and anodyne and stimulating liniment was applied.

As there was constipation, a mild aperient was administered. At 7.30 p.m. on the same day the horse was reported by the manager of the farm to be worse. In my absence, my assistant, Mr. O. G. Barrow, M.R.C.V.S., a most diligent observer, went over, and, after a careful examination, found that no further symptoms of serious consequence had supervened. The case was, perhaps, slightly better. The temperature was 104·5°, and the pulse 84.

On the following morning I found the animal to be very much better. The pulse was 72, and the thermometer recorded 102·3°. During the previous night the animal had drunk his gruel, and eaten a little watery mash.

There was no perceptible discharge from either the nose or mouth, and the horse swallowed his gruel and took his medicine easily. At five o'clock, p.m., on the same day the case began to get worse; the tongue commenced to swell rapidly. On the 15th I found the case to present every symptom of Glossanthrax. The tongue was enormously engorged, and protruded about six inches from the mouth. The surface was of a yellowish brown colour, and the sides were of a purplish-red, and in some parts almost black. The saliva was of a rusty reddish tinge. (The citron-yellow secretion mentioned, so characteristic in the former case, I did not observe in this animal.) The under parts of the tongue presented large excoriated and gangrenous patches. The whole of the head was swollen and œdematous (the swelling did not pit on pressure, but was remarkably firm), especially around the nares and about the eyelids. The submaxillary space and the whole of the throat were enormously swollen. The pulse was almost imperceptible, fluttering, and irregular. The temperature was 105 3° F. The respirations were laboured and roaring, and the expired air was intensely fœtid, so much so, indeed, that it was only with great difficulty that the attendant could be induced to come into the box with me.

The animal was then shot. After an examination the carcase was buried. As far as we are able to judge, the only point of resemblance between Glossanthrax, Anthracoid Angina, and Anthrax Fever, is the intense malignity of the two diseases.

Beyond this, they are in no way related, as far as we can judge. It is therefore difficult to see why the name Glossanthrax or Anthracoid Angina should be retained. The disease is not like Anthrax, and therefore the term *Anthracoid* is inaccurate. The term Malignant or Septicæmic Glosso-laryngitis would be preferable. Another reason for discarding the terms *Anthracoid* Angina and Glossanthrax, is that Black-leg of cattle is also called anthracoid, and thus a kind of intimate relationship might be supposed to exist between the diseases which does not exist.

We are only speaking of horses; we do not here give an opinion concerning the so-called *anthracoid* diseases of cattle.

In the flesh, tissues, and exudations in this case we have found rod-shaped bacilli, not unlike those found in malignant œdema. At present, of course, it would be impossible to state that this bacillus is satisfactorily proved by us to be the cause of Glossanthrax, because only the first of Koch's conditions has been fulfilled. That bacilli have been found in portions of the tongue we can definitely state. Koch's conditions are: (1) It is absolutely necessary that the micro-organisms in question be present either in the blood or in the diseased tissues of man, or of an animal

suffering, or dead from the disease. In this respect, great differences exist, for in some infectious diseases the micro-organisms, although present in the diseased tissues, are not present in the blood; while in others they are present in large numbers in the blood only, or in the lymphatics only. (2) It is necessary to take these micro-organisms from their nidus, from the blood or the tissues as the case may be, to cultivate them artificially in suitable media, *i.e.*, outside the animal body, but by such methods as to exclude the accidental introduction into these media of other micro-organisms; to go on cultivating them from one cultivation to another for several successive generations, in order to obtain them free from every kind of matter derived from the animal body from which they have been taken in the first instance. (3) After having thus cultivated the micro-organisms for several successive generations, it is necessary to re-introduce them into the body of a healthy animal susceptible to the disease, and in this way to show that this animal becomes affected with the same disease as the one from which the organisms were originally derived. (4) Finally, it is necessary that in this new animal affected in this way, the same micro-organisms should again be found. A particular micro-organism may probably be the cause of a particular disease; but that really and unmistakably it is so, can only be inferred with certainty when every one of these *indispensable conditions* of proof has been satisfied.

The writers having but little time for pathological investigation, must apologise for the incompleteness of the above observations, but trust they will be of some utility.

CASES OF SUPPOSED HORSE-SICKNESS IN NATAL.

THE following correspondence, written during last year, from C. Rutherford, A.V.D., to the Inspecting Veterinary Surgeon, Bechuanaland Field Force, is of interest in connection with the serious Horse-Sickness of South Africa, and the investigation about to be made into its nature and prevention:—

SIR,—I have the honour to report, for the information of the Principal Veterinary Surgeon, upon five troop horses of the 6th Dragoons, that died during the month of March, and the three cases of Bilious Fever entered in the register of sick and lame horses.

The 6th Dragoons marched from Vryburg to Mafeking in three lots of one squadron each, the first leaving on March 6th, the second on the 7th, the third on the 8th. At Leawpau and Groot

Choing (one or two days' march from Vryburg) there were large pools and swamps, caused by the recent rains, at which the horses had to be watered—there being no running stream—and where the dew fell very heavily at night. Besides those cases of fever entered in the register, there were many others so slight that, in my opinion, were not serious enough to keep on the sick-horse lines, and which rapidly recovered in two or three days with gentle febrifuge medicine. I beg most respectfully to bring to your notice that all those cases of fever (excepting case 60, E. 33, which died at Vryburg, where he had been left attached to the head-quarter staff) occurred in the following manner:—The first attacked were those that came up with the first squadron, the next lot from the second, and so on, which points, in my opinion, to the pools and swamps mentioned as having been the source of infection. At the outset all showed similar symptoms, and the majority quickly recovered. Those that died developed marked symptoms of Anthrax Fever, excepting case 53, A. 12, which was found dead in the troop-horse lines in the early morning of the 19th March. Therefore I have come to the conclusion that these horses which suffered from what is usually called Bilious Fever, were affected with a mild form of Anthrax Fever.—I have the honour to be, sir, your obedient servant,

C. Rutherford, V.S., A.V.D.

Camp at Mafeking, Bechuanaland, *4th April*, 1885.

Forwarded to the P.V.S.

This occurrence of slight cases of fever, many of which recover, and those that die showing symptoms of Horse Sickness, has previously been reported from Natal. I am inclined to believe they are exceptionally mild attacks of Anthrax. This opinion is also held by Colonial Veterinary Surgeon Wiltshire, of Natal. These cases were noted in Natal, with the view of ascertaining if an immunity from a second attack were given to these horses. But, owing to the unavoidable moves of troops, I am not in a position to give a decided opinion. Should they have an immunity from a second attack, they would be "salted" horses, and their value much increased. These cases are also worthy of note with the view of experimental inoculation. F. Duck, I.V.S., A.V.D.

Mafeking, *April 4th*, 1885.

Editorial.

ANIMAL DISEASE LEGISLATION.

We move slowly in our efforts to combat the contagious diseases of animals —far slower than our necessities would demand; yet it is something to know that we are not quite standing still. Quite recently four Orders of Council have been issued which will give some satisfaction to those who are thankful for small mercies, as they afford a prospect of more vigorous, if still disjointed action; and two serious diseases which have not hitherto been recognised by the Legislature, are now included in the list of those against which sanitary measures can be enforced. A rather important alteration in the rules for dealing with Contagious Pleuro-pneumonia is that "any local authority in Great Britain may, with the view of preventing the introduction of Pleuro-pneumonia into their district, make, from time to time, such regulations as they think fit for prohibiting or regulating the movement by land or by water of cattle into their district from the district of any other local authority in England or Wales, or Scotland; provided that the power to make regulations under this article shall be exercised only by a local authority or their executive committee, and shall not be deputed to any other committee nor to a sub-committee."

Though this change admits of districts being closed to the introduction of cattle from those which are infected, yet it leaves to local authorities permissive powers to stamp out the disease; and so long as this is the case, it is hopeless to expect to see its extinction. This can only be attained by having all the power vested in one central authority, the Privy Council, so as to ensure prompt, energetic, and uniform action; and, as with Cattle Plague, compensation for slaughtered cattle should come out of imperial funds.

One of the Orders deals with Anthrax. This malady is declared to be a disease for the purposes of certain sections of the Act of 1878. Under this Order any person having in his possession or under his charge an animal affected with Anthrax has to keep that animal separated from all animals not so affected, and must give notice, with all practicable speed, to a constable of police in the district where the animal so affected is. The constable is directed to give information at once to the inspector of the local authority, who is bound to report the same to the local authority. The person in charge of the animal may, if he thinks fit, instead of giving notice to the constable, give notice at once to the inspector of the local authority. The local authority are to give the public warning of the Anthrax in any building, field, or other place, and the local authority may, from time to time, make such regulations as they think fit for prohibiting or regulating the movement out of any building, field, or other place, of an animal affected with or suspected of Anthrax, and for prohibiting and regulating the movement of an animal that has been in contact with animals affected with or suspected of Anthrax. The carcase of every animal that has died of Anthrax, or that has been slaughtered in consequence of being affected with Anthrax,

must be buried as soon as possible in its skin; or the local authority may, if authorised by the Privy Council, cause the carcase to be disinfected; and thereafter taken, in charge of an officer of the local authority, to a horse-slaughterer's or knacker's yard, approved for the purpose by the Privy Council, and shall there be destroyed by exposure to a high temperature or by chemical agents.

The fourth is the Rabies Order, which compels the local authority to obtain statistical information regarding the prevalence of this canine scourge in their district, reporting to the Privy Council. It is something to have this disease at last recognised as requiring public notice. We shall now probably learn to what extent it prevails, and the damage it occasions; but, as with Pleuro-pneumonia, its suppression is left to the vacillating, unequal, and perfunctory action of the local authorities; whereas it should be grappled with by the central power, who alone can ensure its general extinction in the United Kingdom, and prevent its reappearance when stamped out.

DOES THE USE OF SALT ON SNOW ACT PREJUDICIALLY UPON HORSES' FEET.*

BY PROFESSOR DR. SIEDAMGROTZKY AND DR. BORN.

IT is generally known that tramway companies use salt to dissolve snow, and thus to clear the rails for their carriage-wheels. This custom has often given rise to doubts and even censure, because the generated brine acts prejudicially upon the feet of animals, more particularly on those of horses, causing Stearrhœa, inflammations of lymphatic vessels, and softening of the hoof.

This reproach, already strongly made as early as 1860 in America, has been frequently repeated elsewhere. The general meeting of the German Jobmasters' Association held at Leipzig on the 11th September, 1885, unanimously passed the following resolution:—"The general meeting of German Jobmasters denies that it is necessary to spread salt to free tram-rails from snow, and declares it to be the duty of the authorities to indicate other means which shall not interfere with general interests; especially as the brine thereby engendered acts undoubtedly harmfully upon stock, both living and dead." To this declaration was added a petition to the imperial sanitary office, asking it to consider the resolution, and to forbid by a general ordinance the use of salt.

No one is in a better position to decide whether salt-spreading acts prejudicially upon animals than the tram companies, for their horses have to move constantly in the brine when salt is used, whereas other animals have opportunities of escaping. Notwithstanding this fact, one hears nothing of the observations made by the companies.

The only remarks in veterinary literature upon the damaging effects caused by salt, are from District Veterinary Surgeon Prietsch, who reports that in the winter of 1884, it was the general belief in Leipzig that the frequent occurrence of Stearrhœa and sores on the extremities of horses was due to immoderate strewing of salt on the tramways. In answer to a request to give further particulars, Mr. Prietsch added that generally, but not always, the affection took the form of gangrenous Stearrhœa (Brandigen Mauke). There was a great deal of snow during the winter.

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It was noticed that although a great number of tram horses were affected, others also suffered, particularly post horses and heavy draught horses. The outbreak did not always follow immediately upon a fall of snow, but more often four, five, or six days after. The salt, which was mixed with petroleum, was strewn not only on the rails, but also between them. In those days the hair was closely shorn off the fetlocks. During last winter, which was not very snowy, however, the hair was only trimmed with scissors, and the salt strewn on the rails only, and not so extravagantly; fewer cases of cracked heels occurred.

As these isolated observations were opposed to those made upon tram-horses, and were not sufficient to decide the burning question of salt-strewing, we endeavoured to attack the subject experimentally, at least as far as horses were concerned. The experiments of Jelkmann ("Rundschau auf dem Gebiete der Thiermedecin," 11, No. 5), did not come before us until we were protocolling our results. Although our object was directed to show the disadvantages of salt-strewing, it should also be mentioned, before we communicate our experiments, that there are other momenta besides the actual strewing of salt which have to be considered. There is the cold thereby generated, and the action of salt denaturalised by mixture with other substances.

It is known that by mixing one part of common salt with three of snow a temperature as low as $-17°$ R. $-(-21°$ C.), may be obtained. If, therefore, the use of salt causes such a fall of temperature, it is easily conceivable that it must act prejudicially upon the health of the animals moving in it; but numerous experiments show that the use of salt upon snow, as practised by the tram companies, never produces such extreme temperatures. The lowest temperature observed was 10° R. (12·5° C.), which did not last more than from half-an-hour to one hour, and then a rise took place. If salt is thrown in moderate quantities (as done by the tram companies) upon freshly fallen loose snow at a temperature of $+2°$ to $3°$ R., at which snow generally falls, then at the utmost result is a fall of only $-3°$ to 7° R. (4° to 9° C.) occurs, according to whether the snow is damp and hard, or otherwise. It will also depend upon these data whether the fall of temperature will take place rapidly or slowly, and becomes equalised again in a short time (two hours), or a long time (four to five hours). If there is brine in the rails and fresh snow falls upon it, then the temperature sinks rapidly to $3°$ to $5°$ R. only, and becomes equalised comparatively quickly.

If it is now considered that salt-strewing during a snow-fall takes place at a moderate temperature, and that the thereby engendered fall of temperature is no greater than 7° to 9° R. (9° to 11° C.) as a rule; if it is further taken into consideration that the salt dissolves the snow which cannot adhere to the hair, but runs off it, and that therefore the cooling mixture is principally applied to the hoof, then it will be recognised that the cold artificially engendered cannot have a prejudicial influence.

At least the effect cannot be worse than that brought about by horses standing hours and days in deep freezing snow.

The mixtures used for the purpose of adulterating salt can have no prejudicial effect. In consequence of the law of 1st September, 1872, which removes the duty on salt used for agricultural and manufacturing purposes, the salt sold for those purposes is adulterated with certain substances. That used by the tram companies is cattle salt, which contains oxide of iron one quarter to three-quarters per cent., or manufacturing salt, which is adulterated with petroleum one quarter per cent. Now as a saturated solution of salt contains only 37 parts of salt to 100 of water, the strength of the added material will then only be 0·09 per cent., which can have no irritating effect upon the skin. The brine is the only cause of harm, if any, to the skin, hoof, or wounds.

A few of the experiments for the purpose of deciding the question were undertaken by Siedamgrotzky, in the Veterinary School at Dresden, and are marked S.; but the greater number were, by permission of the Great Berlin Tramway Company, made by Born (marked B.), with the assistance of Veterinary Surgeons Breune, Kam, Rackow, and Luckhau, upon horses belonging to the company. They were commenced in December, 1885, and closed in the middle of February, 1886. The salt used was mostly adulterated with petroleum, but a certain amount of salt adulterated with oxide of iron ("cattle salt") was also employed.

The recorded experiments refer to the effects of saline solutions in different concentrations upon the unwounded skin of horses.

Experiment 1.—A well-bred horse, with thin skin, had sponges saturated with a solution of petroleum-salt (1 to 5) bound round the heel of the near fore and hind fetlocks. About every three hours the sponges were wetted. Notwithstanding this application for seven full days, no inflammation nor irritation was apparent (S).

Experiment 2.—The same horse had a saturated solution of cattle-salt, on sponges moistened every three hours, applied to the off fore and hind heels. After eight days there was no inflammation nor irritation, merely slight desquamation of the outer layers of the epidermis (S).

In the following experiments upon 182 horses of the Great Berlin Tram Company, the feet, up to the fetlocks, were placed in solutions of petroleum-salt contained in coolers arranged for both fore or both hind, or for all four feet. The time, as the case might be, they remained in the mixture varied from three to twelve hours daily, and in the case of 106 horses the experiment was continued from one day to thirty, and in seventy-six cases for fifty-seven days.

The temperature of the solution varied from + 3° and 6° R., but in some cases it was brought very nearly to freezing point by the addition of snow or ice. Fifty horses had the fetlocks closely clipped before the experiment.

Experiments 3 to 42.—Stood in 10 per cent. solution at first for three hours a day, which was gradually increased to eight hours for fifty-seven days. Forty horses of different breeds, with the hair of fetlocks clipped and unclipped. At the end of the experiment, no change was noticed in the skin nor the hoof of the horses (B).

Experiments 43 to 71.—Stood in 15 per cent. solution, at first three hours, gradually increased until, during the last two weeks, the horses stood twelve hours a day for fifty-seven days. Twenty-nine horses: no bad effects noticed to hoof or skin (B).

Experiments 72 to 96.—Stood in 20 per cent. solution daily, at first for three hours, gradually increased to eight. Time: twenty, thirty, forty, and fifty-seven days. Twenty-five horses: nothing noticed (B).

Experiments 97 to 170.—Stood in 25 per cent. solution, twelve horses, of which four with white fetlocks, and ten greys, daily six hours for nineteen days (B).

Experiments 171 to 184.—Stood in 30 per cent. solution, daily six hours: fourteen East-Prussian remounts, five to eight years old; time, fifty-seven days. Neither during the experiments nor afterwards were any inflammatory changes noticed in the skin or hoof (B).

Experiments 185 to 199.—On the 3rd January, early, some snow had fallen, and it had been strewn with salt. Sufficient of this mixture was dug out of the tram-rails to fill two coolers, which would cover over the fetlock. A brown mare, a chestnut horse with white fetlocks, and a dark-grey stood in these mixtures for six hours a day for from five to eight days. No bad results followed. No results followed upon standing twelve horses in similar mixtures, similarly collected, from 21st to 28th January (B).

Experiments 200 to 214.—Fifteen horses stood six hours daily, from 1st to 15th February, 1886, in a mixture of 20 per cent. salt solution with a great deal of common street dirt. During the experiment they were removed from the mixture, and, without any cleansing, sent to work. Nothing untoward was noticed (B).

Experiment 215.—A coarse-bred horse, with moderate tufts on its fetlocks, had sponges fastened to the fetlocks for fourteen days. The sponges were wetted every three hours with a solution of salt in snow. At the end of the experiment no increased sensibility or inflammatory appearances were noticed (S).

Experiment 216.—The near hind foot (white) of a coarse-bred horse was placed in a concentrated solution of petroleum-salt in plain water. A quantity of undissolved salt remained at the bottom of the vessel. The horse stood in the solution from seven in the morning until six in the evening for seven days. Nothing occurred (S).

Experiment 217.—The off hind foot of the same horse, upon which was an old scratch covered with scab, was placed in a solution of petroleum-salt in snow-water. Beyond gradual softening and desquamation of the scab, under which a normal epidermis was built up, nothing occurred to the foot or the skin. Time, seven days (S).

It will be apparent from these numerous observations, that solutions of cattle and other salt of different strength exercise no prejudicial influence upon unwounded skin, nor upon the hoof; it does not engender cracked heels or Stearrhœa. This holds good not only for pure solutions, but also for those made in snow water, and in muddy snow water collected from the rails of the tram-lines. The great number of negative results with horses with white legs, therefore of the more irritable description, shows that these results were with horses most likely to be attacked. Some of these were rendered even more liable by having the fetlock kept closely clipped, and others had formerly suffered from Stearrhœa.

That the brine should act prejudicially upon wounds was not to be expected, for solutions of salt to a certain extent disinfect, and are often used by Lisfrance and others as a cleansing lotion for neglected sores. Nevertheless, some experiments were instituted to settle the question.

Experiment 218.—A wound, about as large as a shilling, was made upon the outer side of the near fore fetlock. The foot was placed in a saturated solution of "cattle" salt, from 7 a.m. till 6 p.m. The animal showed slight pain at first only, but did not remove the leg. The wound was covered in three days without any complication (S).

Experiment 219.—An incised wound, 5 cm. long, but not quite penetrating the skin, was made on the near hind foot. The leg was put in saturated solution of salt daily for 11 hours. There were no wound complications, and the place healed in four days (S).

Experiment 220.—An oblique incision, 5 cm. long, was made on the off hind fetlock through the skin. The foot was placed in concentrated solution of salt. On the third and fourth day there was slight œdema in the neighbourhood of the wound, extending half-way up the cannon bone. On the eighth day the wound was healed, except a small scar. No complications occurred (S).

Experiments 221 to 225 were conducted upon horses which had been recently fired or blistered, and all gave negative results.

Experiment 226.—A Danish horse had a mallender on the near hind leg (white). The exudation extended up to the hock. The sensitiveness of the skin was so great that the horse nearly fell when forced to move. The leg was placed in a solution of salt (25 per cent.), and healed without complications, and quite as rapidly as if other suitable remedies had been employed (B).

Experiment 227.—A grey with mallenders on both legs, swelling extending to the hock, great exudation in the neighbourhood of the fetlock. The case was treated with 20 per cent. solution of salt, and was cured in ten days (B).

Experiment 229.—A six-year-old grey had suffered for eight weeks from a quittor which was not cured by the ordinary remedies, including blisters and firing. It was placed in a 15 per cent. solution of salt for eight hours daily, and discharged cured in three weeks.

* *Experiment* 235.—A bay suffering from Acute Laminitis was placed in a 20 per cent. solution of salt. In three days all the symptoms of the disease had disappeared (B).

The above experiments prove that solutions of salt of different strengths have no prejudicial influence upon wounds, skin inflammations, cracked heels, Stearrhœa, fistula, etc.

In no case did irritation of any importance, or wound complications (Erysipelas, Lymphangitis) occur; on the contrary, the favourable influence was to be noticed. Although no changes were observable in the horn which had stood in brine, nevertheless the following experiment was conducted.

Experiment 236.—The hoof of an anatomical subject which had been slaughtered three days previously was obtained, and pieces as nearly as possible of similar size were cut from the walls, sole, and frog. The soft textures were removed. The other parts were weighed. Of these some (No. 1) were placed in saturated solution of salt, and others (No. 2) in water. After three days they were weighed with the following result:—

	Original Weight.	Weight after Experiment.	Increase or Decrease of Weight.
Wall Horn 1, Solution of Salt ...	29·05 gr.	29·03 gr.	− 0·07
,, 2, Water	28·30 ,,	28·67 ,,	+ 1·32
Sole Horn 1, Solution of Salt ...	13·75 ,,	12·40 ,,	− 9·82
,, 2, Water	12·85 ,,	14·01 ,,	+ 9·03
Frog Horn 1, Solution of Salt ...	14·15 ,,	12·48 ,,	− 11·80
,, 2, Water	14·33 ,,	16·90 ,,	+ 17·94

(S).

From the above it will be seen, that whereas horn placed in water constantly acquires water, horn placed in a saturated solution of salt loses water. It follows, therefore, that solutions of salt do not cause softening, but hardening of the hoof. It may be concluded from this experiment, that no softening of the hoof is caused by salt-spreading. On the contrary, the brine renders the horn more capable of resisting the softening influence of thawing snow and water.

As a corollary, the following experiments upon dogs are mentioned, because it is not infrequently stated that they suffered from inflammation of the feet by running in the brine on the tram-lines.

Experiments 237 and 238.—A dog's foot was placed in super-saturated solution of salt for two hours and a half. The result was negative. The experiment was repeated, and confirmed the above.

From the experiments it may be deducted with certainty, that the complaints against the use of salt to dissolve snow, in so far as the complaints are connected with the feet of horses, have no foundation in fact, and rest principally upon surmise. Our negative results agree, completed with those

* The above are selected from a number of cases, all of which have a negative termination as regards the question at issue.

of Jelkmann (*l. c.*) As often happens, the blame is wrongly put upon the most apparent cause, whereas in reality, the cases of cracked heels, Stearrhœa, and Lymphangitis, which follow upon scratches in winter, and in muddy and snowy weather, are caused by too late clipping of the hair of the heels, insufficient cleanliness of the heels, and the action of wet straw. It is acknowledged that trimming, particularly close clipping, late in the season is an important cause of disease. But it has been demonstrated in large stables, that moderate clipping of the heels early in autumn has reduced the occurrence of Stearrhœa, etc., to a rarity.

PASTEUR AND HIS WORK, FROM AN AGRICULTURAL AND VETERINARY POINT OF VIEW.

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(Continued from page 273.)

In my work on "Veterinary Sanitary Science and Police" (vol. ii., p. 208), I have described a disease affecting poultry, which, from some of its symptoms, and from its sometimes occurring in this century coincidently with human Cholera, has received the name of "Fowl Cholera." In my "History of Animal Plagues," however, I have shown that it has no relationship, in point of time, with the terribly fatal scourge of mankind; and in the first-mentioned work, I have demonstrated that it differs widely from it in its pathology. It has been known from a very early period in Europe, and it has appeared in India and on the American Continent—everywhere manifesting itself as a very rapidly destructive disorder. Since 1798, it has been studied and described in Italy, Germany, and France by veterinarians and others.

Delafond, who first saw the Anthrax microbe, believed it to be that disease; in 1873, a veterinary surgeon of Alsace, Moritz, discovered a special microbe in the blood of diseased fowls; and in 1878, Professor Perroncito, of the Turin Veterinary School, described this organism; while Professor Toussaint, of the Toulouse Veterinary School, also recognised it, and sent Pasteur the head of a bird that had succumbed to the malady. It had, during this century at least, been considered a most virulent disorder, readily communicable to healthy fowls by cohabitation, by inoculation, and by feeding them with food or water tainted by diseased birds.

Pasteur cultivated the germ in fowl-broth, in which it multiplies in a wonderful manner, as in a few hours the clearest decoction becomes turbid, and is found to be swarming with extremely small organisms, constricted in the middle, and motionless; but in a few days they change into a lot of isolated specks so minute, that the fluid, from being almost milky white, again becomes almost as clear as it was at first. The organism was found to belong to the kind called "micrococci"—in which class Pasteur predicted the microbes of the viruses yet unknown would probably be found. He observed that yeast-water, in which so many diverse microscopic organisms find suitable nourishment, was quite unsuitable for the growth of this microbe—indeed, it perished in it; and that the fowl-broth was the only medium in which it really flourished. The thousandth part of a drop of this mixture inoculated in a fowl would cause death; but in guinea-pigs only a small abscess appeared at the point of inoculation, and this opening spontaneously, a little pus, teeming with microbes, escapes. The virulence of this pus is extreme, and if fowls are inoculated with it they will perish quickly;

but the guinea-pig suffers no disturbance in health. Rabbits appear to be as susceptible almost as fowls; so that if the pus of the abscess were smeared over the food of fowls and rabbits associating with the guinea-pig, these would die, but the latter would remain as well as ever. The excrement contains the organisms in great abundance, and it is chiefly through this that the disease is propagated in poultry yards.

Repeated cultivations do not diminish the activity or potency of the organism of Fowl Cholera, which is ærobic, and cultivable in air or aerated fluids; unlike that of Anthrax, however, which when excluded from the air, in a few days disappears or is reduced to fine amorphous granulations, this may be kept for years without air, and still remain active.

In experimenting thus with Anthrax virus and that of Fowl Cholera, Pasteur was on the eve of his greatest discovery—the attenuation or enfeebling of the virus of contagious diseases, by which the bane could be made to serve as its antidote in the living body. The successive steps by which he arrived at an intimate knowledge of the real agents at work in the production of these diseases, will be observed to be closely related to each other, as each was the rigorous verification, by conclusive experiment, of a preconceived idea, upon which he always worked. "Nothing can be done," he asserted, "without preconceived ideas; only there must be the wisdom not to accept their deductions beyond what experiment confirms. Preconceived ideas, subjected to the severe control of experimentation, are the vivifying flame of scientific observation, while fixed ideas are its danger."

The fact that certain contagious diseases, as a rule, affect an animal or a man only once in a lifetime, has been known from time immemorial; and this fact of non-recurrence has been acted upon in the case of some of these disorders, in producing them purposely in man and beast, in a milder form if possible, or, at any rate, under more favourable conditions, than in the ordinary accidental or natural manner. Eastern people—the Chinese, for example—looking upon an attack of Small-pox as inevitable, and knowing the great mortality or disfigurement caused by the disease when caught through infection, have resorted to inoculation in order to induce a less serious and more tractable form at a convenient time. With the same object, for very many years, inoculation has been practised for Sheep-pox in some of those countries—as Germany and France—where it always prevails to a more or less considerable extent. In the Russian Steppes, also, for a long time experiments were made to test the advantages of inoculating cattle for Rinderpest; and in recent years the same measure has been tried for "Distemper" in dogs. In some outbreaks of Foot-and-mouth Disease, cattle-owners have often produced this troublesome affection in their yet healthy animals, in order to prevent it lingering among them for a long time, and so to get rid promptly of what threatened to be an inevitable and more harassing visitation. And protective or preventive inoculation for the Lung Plague of cattle, introduced by Dr. Willems, of Hasselt, some thirty or forty years ago, has been very much resorted to in countries where that insidious and deadly pest prevails. But in all these instances the inoculation has been effected with the natural, or what we might designate the "crude" virus; and therefore the danger was great that the diseases might at times be induced in as great virulency as when they appeared in the ordinary way. This has been the case especially with Cattle Plague inoculation, which, because of the unsatisfactory results, has now been abandoned; while the benefits to be derived from Sheep-pox inoculations are, perhaps, more than counterbalanced by the disadvantages which attend them.

The introduction of vaccination as a protection against Small-pox, by Dr. Jenner, has proved an immense success, but hitherto it has been an unique instance of one form of disease proving an antidote to another form. Cow-

pox is not the same disease as Small-pox, and by no known means can it be transformed into the latter; neither is Cow-pox human Small-pox changed in almost every characteristic feature by transmission to the cow. Physicians and others have made the strange blunder of considering the two diseases as dependent on the same virus, notwithstanding the most obvious reasons to the contrary; and this blunder is repeated in medical books. But it is a patent fact, nevertheless, that Cow-pox virus, when inoculated in man, protects him from Small-pox, which the Pox of no other animal will do, that I am aware of, except it be that of the camel and horse. The microbes of Cow-pox find a congenial soil in mankind, multiply there, and make that soil unsuitable for the microbes of Small-pox.

Why one attack of a contagious disease should render the body of a creature proof against another, for a more or less extended period, or even for the entire duration of life, is not at present known. Various hypotheses have been presented at different times to account for it, but that which has perhaps received most favour is the one that explains the immunity, by suggesting that something has been removed from the blood in the first attack which is necessary for the growth and multiplication of the disease-producing germs, and which is only slowly, or never reproduced. In the words of Professor Tyndall, "When a tree or a bundle of wheat or barley-straw is burnt, a certain amount of mineral matter remains in the ashes, extremely small in comparison with the bulk of the tree or of the straw, but absolutely essential to its growth. In a soil lacking, or exhausted of the necessary mineral constituents, the tree lives, the crop cannot grow. Now contagia are living things, which demand certain elements of life just as inexorably as trees, or wheat, or barley; and it is not difficult to see that a crop of a given parasite may so far use up a constituent existing in small quantities in the body, but essential to the growth of the parasite, as to render the body unfit for the production of a second crop. The soil is exhausted, and, until the lost constituent is restored, the body is protected from any further attack of the same disorder. . . . . To exhaust a soil, however, a parasite less vigorous and destructive than the really virulent one may suffice, and if, after having by means of a feebler organism exhausted the soil without fatal results, the most highly virulent parasite be introduced into the system, it will prove powerless." Another theory, highly favoured by some eminent authorities, is to the effect that the microbes, during their multiplication in the body, cause the production of some substance which is detrimental to a second invasion of them. This theory would seem to have good foundation in what is known to occur in fermentation. The poisonous substance may remain persistently in the blood and tissues, in which case immunity will be permanently established; or it may be removed in the course of time, and the body become again susceptible, and the protection is only temporary. Both theories are perhaps equally acceptable, in explanation of what occurs.

Vaccination, as a defence from Small-pox, seems to have led Pasteur to ask himself why, if contagious maladies do not recur, there should not be found for each of them a different disease, but resembling them, which acting upon them as Cow-pox does upon Small-pox, would prove preventives of them? And what has been called a "chance occurrence" appears to have thrown open the way to enable him to give a reply to the question. Having shown that the microbe of Fowl Cholera could be cultivated in an artificial medium for hundreds of times, without its virulence being diminished in the slightest degree, he found that this virulence could only be assured when no great lapse of time had occurred between the successive cultivations —the second culture being sown twenty-four hours after the first, the third twenty-four after the second, and so on to the hundredth or more. When several days, weeks, or even months, were allowed to elapse between the

cultures, a progressive weakening in power of the microbe was apparent. So that if fowls are inoculated with the successive cultivations made at short intervals, and die in the course of one or two days, those which are inoculated with that which has been made at an interval of some weeks or months, will suffer much less and recover. But the most startling phenomenon in this respect is, that after recovering, should they be re-inoculated with the most active virus which would kill its hundred per cent. of those inoculated with it in a few hours, they will not die, perhaps even show no signs of illness; they are protected by inoculation with the delayed or attenuated cultivation.

The discovery of this power in the weakened microbe of a contagious disease, to protect an animal from the lethal action of the same kind of microbe when in full virulency, must be held to constitute the most important of Pasteur's service's to medical science and to mankind; inasmuch as its effects may be of the greatest magnitude, and far-reaching, when made applicable to the many contagious disorders—so deadly and so little under the control of man—which affect our own species and the lower animals. The destructive microbe is transformed in the laboratory, by the skill of man, and at will, into a benign, life-preserving agent. In the case of the Fowl Cholera germ, the attenuating influence in operation was merely the oxygen of the air; and the proof of this is afforded by cultivating the germ in a tube having only a little air. If the tube be hermetically sealed, the microbe soon consumes the oxygen in the air and in the culture-fluid, and retains its potency for evil unimpaired, for months, or even for years. The reason why the oxygen of the air has no influence upon it during the twenty-four hours' cultivation, is explained by this being required for its nourishment and reproduction; after that time, the air gradually modifies or weakens it, until at last its power is annihilated, though the organism still lives. The experimentalist can in this way reduce, as he wills it, a virus to any degree of virulency. Pasteur could inoculate fowls with a cultivation too feeble to protect from the deadly action of the crude virus, but effectual in insuring them against a stronger cultivation. A second cultivation would nullify the action of a third, and so on until the most deadly virus was rendered inert in the bodies of the fowls. The whole secret of this protective inoculation consists in knowing at what moment a certain degree of virus attenuation is a guarantee of safety against the full-power virus.

Trials of the attenuated virus of Fowl Cholera were made by veterinary surgeons and agriculturists, and with perfect success in every instance. It was found necessary to inoculate each fowl twice—at first with a very weak virus, yet sufficiently strong to prevent dangerous consequences from a stronger; then in ten or fifteen days with a less attenuated one. The preparation of these cultivations—or "vaccines," as Pasteur designates them, in honour of Jenner, as it is obvious they have nothing to do with cows in this and some other animal diseases—demands much care in manipulation and experimentation, in order to arrive at the proper degree of harmlessness and potency. Species, and even breeds, and likewise individual peculiarities, have to be tested as to receptivity, before the method can be generally applied.

When Pasteur laid before the Academy of Sciences the details of his new discovery, the importance of its bearings on medicine—human and veterinary—as well as on medical doctrines, was fully recognised by those who were competent to form an opinion, and by no one more enthusiastically than the eminent veterinarian, M. Bouley, director of the French Veterinary Schools, and whose death last November, while President of the Academy of Sciences, Medicine, and Agriculture, we must deplore. "This is but a beginning," he exclaimed; "a new doctrine opens itself in medicine, and

this doctrine appears to me powerful and luminous. A great future is preparing ; I wait for it with the confidence of a believer, and with the zeal of an enthusiast."

Having succeeded so remarkably with the plague of fowls, Pasteur was naturally desirous of extending his protective inoculation to other diseases. Anthrax was at that time being investigated in his laboratory ; but as nothing was known as to whether one attack of the disease gave exemption from another, no idea could be formed whether inoculation would be successful. People who are accidentally infected from diseased animals rarely recover, and when they do, a second infection is not likely to be observed ; sheep attacked with it generally all perish ; but bovines are more resisting, and recoveries are not unfrequent. An opportunity occurred in 1879, in a veterinary surgeon of the Jura bringing forward a cure for Splenic Fever, when Pasteur was deputed by the Minister of Agriculture to report upon its value. A lot of cows were inoculated with crude virus, and one-half of the number were treated according to the veterinary surgeon's method, the other half receiving no treatment. The pretended remedy was a failure, but though many cattle died from Anthrax, others survived, after being very ill. When they had quite recovered, they were re-inoculated with large quantities of virus, but no symptom whatever of the disease resulted—even when this was done after more than a year had elapsed. The question of immunity from recurrence was therefore settled, and the object now was to find a method of attenuating the virus of this malady, as he had that of Fowl Cholera.

But Pasteur and his able assistants, Chamberland and Roux, met with a serious difficulty at starting. The microbe of Anthrax differs from that of the poultry disease, in reproducing itself by spores, as well as by fission, the latter alone being the mode of generation in the last-named malady ; while the spores may be exposed to the air for years without losing their virulence, and ready to develop into the characteristic rods, or bacilli, whenever transferred to a suitable medium. The virulence of the spore is protected from injury by its impervious covering or shell. The problem therefore was, supposing the filaments or bacilli to be analogous to those of Fowl Cholera, and cultivable as a protective agent, to determine the conditions which would prevent the formation of spores. At length, after many experiments and anxious consideration, success was secured. It was found that at a temperature of 111° to 114° the Anthrax microbe could not be cultivated, but that this could be easily done at 108° or 109°, at which it produced no spores. At that temperature, therefore, and in contact with pure air, cultivation of bacilli destitute of spores can be effected. But in a few weeks the crop perishes, for when sown into a fresh medium there is no result ; though in the preceding days life is still present in them. When the cultivation is exposed to air and warmth for two, four, six, or eight days, and is then inoculated in animals, its activity is found to be modified in proportion to the duration of exposure ; it presents, in fact, a succession, of gradually attenuated viruses, as in Fowl Cholera, each constituting a foil for the uncultivated microbe.

(*To be continued.*)

PASTEURISM.

AT a lecture given in the Sorbonne, Paris, on Sunday, October 10th, by Dr. Chautemps, of Paris, Vice-President of the Paris Municipal Council, on M. Pasteur's method, the lecturer stated that out of a total of 1,583 French patients, only 10 deaths occurred, of which 2 were attributable to accidental

causes. He desired to deal only with the cases under treatment previous to August 1st, in order to leave an ample margin of seventy days. They amounted to 1,276. Ten per cent. of these were bitten by dogs manifestly mad, and in 70 per cent. of the cases the dogs were scientifically shown to have been rabid. The remaining 20 per cent. were doubtful cases. Hence, out of 1,276 persons, 973 were bitten by dogs ascertained to have been mad. In the natural order of things, 155 deaths would have taken place, or 145 over the insignificant rate. Dr. Chautemps was satisfied M. Pasteur had saved 145 lives. Cases of Rabies were of more frequent occurrence than was generally supposed. In the space of five months, 81 cases of dog-bites came to the knowledge of the Paris Prefecture of Police. It was a significant fact that only 10 of M. Pasteur's patients died, while he (M. Pasteur) had heard, almost by accident, of 12 unvaccinated cases ending fatally. Several of his failures might be set down to the long interval that elapsed between the bite and treatment, but even this risk would no longer exist when Pasteurian virus stations were established in Russia, at New York, at Buenos Ayres, and elsewhere.

SHORTHAND NOTES OF TWO LECTURES ON POISONING OF HORSES BY LATHYRUS SATIVUS.

BY PRINCIPAL M'CALL, F.R.C.V.S., VETERINARY COLLEGE, GLASGOW.

GENTLEMEN,—Having in previous lectures considered in detail the nature, causes, and symptoms of the diseases which usually attack the larynx, and lead to roaring and other forms of abnormal breathing, I purpose this morning directing your attention to a specific form of roaring produced by feeding horses on an Indian vetch, recently imported into this country, termed "Lathyrus Sativus."

The cases to which I shall immediately refer, and some of which, I am sorry to say, proved fatal (with one exception), occurred in one establishment in this city and its branches in other towns, and I have not heard of any similar outbreak in Scotland.

History.

1st Case.—An aged horse, the day after arriving in Glasgow from Falkirk, died suddenly. On *post-mortem* examination, the mucous membrane of the pharynx and larynx was found to be irregularly congested; some portions were dark in colour, others red, while patches between them seemed healthy. There was a considerable serous exudation in the submucous tissue, which was irregularly distributed, being much more extensive in some places than others. For some distance down the trachea, the mucous membrane was in a similar condition. At the division of the bronchial tubes, the membrane again became congested, while at the small tubes it was completely black in colour. The lungs were congested, and the bronchial tubes contained a large quantity of liquid, which seemed a mixture of mucus, blood, and air. The heart was not weighed, but was enlarged and pale in colour, and on the left side near to the apex was an irregular patch several inches in diameter, which was dark in colour, and the pericardial sac was also dark in colour, and contained a quantity of fluid. The stomach contained a large quantity of food, which had a most disagreeable odour, and was in a very dry condition. The mucous membrane of the stomach was congested, more so in some parts than others, as if the animal had suffered from some form of gastric irritation.

2nd Case.—A horse from Oak Street, Glasgow, belonging to the same firm, died suddenly on the street while at work. The lungs only were examined, and showed symptoms similar to first case. This animal showed no premonitory symptoms, and the carter considered it had been "worried."

3rd Case.—Shortly after above, another horse from same stable fell on the street while at work, and with difficulty it was got home. It was found to be suffering from palpitation of the heart. It was under treatment for some weeks, but not improving it was sold, and died suddenly on the street while being removed to its new home. There was no *post-mortem* made of this animal.

4th Case.—Shortly after, another animal died during the night; and *post-mortem* showed symptoms much the same as No. 1.

5th Case.—Another horse died in the act of pulling a heavy load up a hill at Rutherglen. This animal was from the West Street stable. It was found in much the same condition as No. 1.

6th Case.—Went to Falkirk and was shown three horses said to be suffering from some disease of larynx and trachea. Two seemed to be in perfect health; the other did not seem so, and was minutely examined. As long as it was at rest the animal seemed in perfect health. The animal was feeding as well as ever he did. But in the language of his driver, he was getting "thick of his wind," and was useless for work. He was brought out of stable and walked; he was then harnessed and put to an empty lorry, and, after going about 200 yards, presented the following symptoms:—Stood with his fore legs forward, neck stretched, and elbows out, and breathing so laboured that he was with difficulty kept on his feet till unyoked. His mouth was kept wide open, and his tongue hung out. Each respiratory movement was accompanied by a loud sound, which seemed to come entirely from the larynx. The animal was sweating profusely all over the body, running down the limbs, and dropping off the abdomen. Pulse very quick, irregular, and intermittent. Impulse of heart very much increased. Venous pulsations present; distinct vibration felt over region of larynx; temperature normal. These symptoms disappeared about as quickly as they appeared. In about five minutes the animal seemed as well as ever, and when put in stable started to eat hay. The other two animals were not so badly affected as this one.

7th Case.—Horse died in horse-box, coming by rail from Stirling to Glasgow, and the *post-mortem* showed that he had been affected in much the same way as No. 1. The heart was much more affected, and stomach much more congested.

8th Case.—Went to Stirling, saw another horse presenting symptoms similar to No. 6 when trotted.

9th Case.—Saw white horse at Stonehaven. When at rest sounds in trachea and larynx much increased; heart very irregular, stopped altogether occasionally; venous pulsation at jugular; impulse of heart increased; internal temperature normal; superficial temperature very irregular; eczematous inflammation of skin at several places; when put to work symptoms much aggravated, and as in No. 6.

10th Case.—Another horse died in stable, and death said to be due to disease of heart.

11th Case.—Mare died on railway while coming from Alloa to Glasgow. Similarly affected.

October 6th.—West Street Stable Stud examined. Out of twenty-four horses thirteen were affected. Some of these only very slightly; in some cases heart only, in others the larynx and trachea.

October 7th.—Oak Street horses examined. Out of twenty-five about a dozen were found affected.

October 8th.—Another animal in Stirling affected and put under treatment. Same day examined several studs at different stations in the neighbourhood, and found twenty-five affected, some slightly, others badly.

October 17th.—Another bad at Stirling, so bad that active treatment alone saved his life.

Cause.

When the disease first appeared at Falkirk, the food and water were at once suspected; but when the disease made its appearance at several other places, the water theory was considered improbable, and the food alone suspected. On inquiry it was found that all the horses belonging to this firm were fed on same variety of chop, which appeared first-class. But at night some of the studs of horses got boiled food, while others instead of boiled food got a *mixture* of several varieties of meal and bran, which was given raw and damped with cold water. The boiled food consisted of barley and turnips, Indian corn, Indian peas, and bran. The chop or *mixture* was made of rough seeds, eight hundredweight; Indian corn meal, roughly ground, five hundredweight; Indian pea-meal, also roughly ground, five hundredweight; bran, two hundredweight. It was found that the *horses fed* upon the *boiled food* were all healthy, while those that had been fed upon the chop mixture alone suffered. It was then thought possible, that the giving of this food imperfectly damp and always raw might be the cause of the malady, but as the mixture had been similarly given for several years, aud without bad effects, why should it be the cause of disease now? It then transpired that the mixture, which had formerly been made of beans and peas grown in this country, had lately been made with pea-meal from a special variety of pea grown in India; and as this pea, from its shrunken appearance, did not look sound, and being mixed up with a large amount of dust and dirt, it was thought that it might be the cause of the malady, and to test the matter the following experiments were tried:—

1. An aged horse was fed solely on a mixture made with the ground Indian pea-meal. About ten or twelve pounds the horse would take in a day. In ten days he showed all the symptoms of the disease when submitted to the slightest exertion.

2. An aged mare was fed on the usual mixture, but bean-meal was substituted for the Indian pea-meal, and she was subjected to the same treatment and remained all right.

3. This animal was fed for several weeks upon the Indian peas raw and whole, but beyond an irregular action of the heart, the animal kept all right. This is an exception to the others. (Indian pea, raw, is apparently much more active when ground and lying in bulk for some time.)

4. Another horse was fed on the peas boiled, but as he refused to take them after the first week (boiled Indian peas having a sickly odour), the experiment broke down.

The first experiment at least proved that the meal made from the peas could produce the disease when given raw.

After these experiments had been completed, the feeding of the various studs with raw meal from the Indian peas was stopped, and the disease gradually disappeared.

Not satisfied, however, with the results of these experiments, Prof. McCall instituted another, the particulars of which are as follows:—

An aged dark-brown cart-mare was purchased and examined as to condition of heart, lungs, and trachea, and, as far as could be ascertained from the external examination, these organs were believed to be sound. This animal, although a good deal worn about the legs, seemed to meet our purpose. It was feeding well, temperature normal, and apparently in health. After examination, the animal was put upon feeding, which consisted principally of the Indian pea, and a small quantity of bran, oats, and cut hay, to encourage her to eat them. She was receiving fifteen lbs. grain—chiefly peas. When we attempted to make her entirely partake of peas, she preferred to eat the straw about her feet, so that we were compelled to take up the straw-bedding and put tan-bark down.

1st day of experiment, pulse, 52; respirations, 14; temperature, 101°
2nd " " " 52; " 15; " 101
3rd " " " 48; " 14; " 100·6

Not feeding well, and very dull.

4th day of experiment, pulse, 48; respirations, 12; temperature, 100.4
5th " " " 50; " 12; " 101
6th " " " 48; " 14; " 101·4

On the floor of the mouth, and inside cheeks and gums, small vesicles and inflamed spots were seen, just as if they had been caused by the stinging of a nettle.

7th day of experiment, pulse, 46; respirations, 14; temperature, 100.4°
8th " " " 84; " 12; " 100·4
9th " " " 86; " 11; " 100·4
10th " " " 82; " 13; " 100
11th " " " 84; " 14; " 100·4

After exercise, the pulse rises to 130, and is both irregular and intermittent; sound of trachea and larynx considerably increased. Vibrations over larynx.

12th day of experiment, pulse, 76; respirations, 16; temperature, 100·6°
13th " " " 70; " 14; " 101
14th " " " 68; " 14; " 100·6
15th " " " 70; " 12; " 100 4
16th " " " 74; " 10; " 101
17th " " " 72; " 12; " 100·4
18th " " " 70; " 11; " 100·4

After exercise the pulse rose to 168, and the respirations 24. The respiratory sounds more increased.

19th day of experiment, pulse, 68; respirations, 12; temperature, 100·6
20th " " " 70; " 13; " 100·4

Respiratory sounds more increased than on previous day. Eruption gone, but a similar condition making its appearance on withers, and spreading along back.

21st day of experiment, pulse, 70; respirations, 10; temperature, 101°
22nd " " " 72; " 11; " 100·4
23rd " " " 78; " 12; " 100·2
24th " " " 74; " 13; " 100
25th " " " 72; " 13; " 100
26th " " " 74; " 12; " 100·2
27th " " " 76; " 10; " 101

Refused to take peas unless when mixed with an equal quantity of oats, and then in less quantity; losing flesh rapidly.

28th day of experiment, pulse, 72; respirations, 12; temperature, 101°
29th " " " 72; " 12; " 101
30th " " " 74; " 12; " 100·4
31st " " " 76; " 12; " 100·4
32nd " " " 78; " 12; " 101

After exercise, pulse is 160. Very weak on limbs. Animal fell at exercise; great difficulty in rising again. When put in box fell down again.

33rd day of experiment,	pulse,	74 ;	respirations,	11 ;	temperature,	101°
34th	„	„	„	76 ;	„	10 ;	„	101·1
35th	„	„	„	78 ;	„	10 ;	„	101·2

Hair now rapidly falling off, and very weak.

36th day of experiment,	pulse,	76 ;	respirations,	11 ;	temperature,	101·4°
37th	„	„	„	78 ;	„	10 ;	„	101·4
38th	„	„	„	78 ;	„	10 ;	„	101·4
39th	„	„	„	80 ;	„	10 ;	„	101
40th	„	„	„	82 ;	„	11 ;	„	102

After exercise, pulse 154; respiration, 28, and sweating on the slightest exertion.

41st day of experiment,	pulse	74 ;	respirations,	10 ;	temperature,	101°
42nd	„	„	„	72 ;	„	10 ;	„	101
43rd	„	„	„	76 ;	„	11 ;	„	100·4
44th	„	„	„	74 ;	„	10 ;	„	101
45th	„	„	„	74 ;	„	10 ;	„	100·4
46th	„	„	„	70 ;	„	10 ;	„	101·4
47th	„	„	„	72 ;	„	9 ;	„	100·2
48th	„	„	„	70 ;	„	9 ;	„	100·4
49th	„	„	„	68 ;	„	10 ;	„	100·2
50th	„	„	„	76 ;	„	11 ;	„	101·4

After exercise pulse is 164 and respirations 26; sounds of larynx and trachea much increased. When hand placed on larynx a distinct vibration was felt. When by pressure this was stopped animal commenced roaring. At this period large patches of skin wanted the hair, and the animal being much reduced in condition, it was deemed advisable to have her destroyed, and which was done the following day by bleeding from the carotid artery.

Post-mortem.

As the skin eruption somewhat resembled that seen in Mange, a microscopic examination was made, but no parasite was found. A similar examination was made several times during life. The abdomen was opened into, and was found to contain half-a-pint of red-coloured fluid; it resembled serum, but the peritoneum was healthy.

The abdominal organs were then removed and presented the following appearance: The stomach was nearly empty and the food had disagreeable acetic odour; the walls much thickened, irregularly congested, in some places nearly black in colour, and other portions healthy; the mucous membrane had lost connection with submucous tissue. The bowels contained a quantity of semi-digested food, and this substance was firmly adherent to the walls, and with difficulty removed. The walls of the intestines, especially the large, were considerably thickened, some places quarter of an inch, and the mucous membrane was in the same state as in stomach. The thorax, when opened into, appeared normal and had no fluid. The pericardial sac was irregularly discoloured, and contained 2½ ounces of fluid, resembling that found in abdomen. The heart was very large, soft, and irregularly marked, and 12¾ lbs. in weight. Examined externally, one side of the heart was lighter in colour than normal; part of other side was much darker. The valves and lining membrane seemed healthy. The lungs presented nothing abnormal, except a small quantity of fresh mucus at division of trachea, but this probably was due to mode of death. The trachea was apparently healthy, the *larynx* was irregularly marked with spots of congestion, and these best marked around the glottis. The muscles of left side were pale in colour and smaller than

those of the right. The liver was darker in colour than normal, soft and friable; the other organs presented nothing peculiar.

The symptoms presented by this animal during life resemble in many particulars those seen in other cases enumerated, and the absence of very marked respiratory disturbance and lesions may be accounted for by the animal being kept at rest, and not allowed to forcibly call into play the respiratory and circulatory apparatus; and, moreover, the animal was killed and not allowed to live until death ensued.

(*To be continued.*)

SHORT NOTES RESPECTING THE ETYMOLOGY OF THE WORD "HORSE," AND THE DEDUCTIONS DERIVED THEREFROM.

BY ALBERT GRESSWELL, B.M., CHRIST CHURCH, OXFORD, AND EMIL SEYTTER, PH.D., GOTTINGEN.

IT is highly interesting to note that the word "horse" is very similar in all Aryan languages, thus showing us that this noble animal was domesticated on the table-lands of Bactriana at a pre-historic as well as a pre-sanscritic period, when the Aryans still dwelt together.

The name in Sanscrit was açva; Zend, açpa; Persian, asp; Armenian, asb; Lithuan, aszwa; Latin, equus; Greek, ἵππος (derived from a primitive form ἵκϝος and ἵκκος, which was only preserved by the Æolians); Gaelic, epos; Gothic, aihvus; Old High German, ehu.

The primitive meaning of the word "horse" was "the rapid animal." All other nations of pre-classical or classical date have no proper word for the horse, but borrowed the Aryan forms.

The art of riding was not known in high antiquity, horses only being employed for chariots. Riding is of more recent date Greece was the home of equitation, which first originated in Thessaly (*cf.*, the myth of the Centaurs).

To the Greeks the art of riding was unknown before the year 500 B.C. At the time of the Grecian supremacy we know that horses were used for the chariots, and about the year 450 B.C. we read of riding and of cavalry in Greece proper. If we turn to Roman history, we learn that in the early Regal times, and in the first centuries of the Republic, cavalry was the chief strength of the military armament.

The word "ass" is purely Semitic in origin, and the word "pig" is Turanic (Egyptian).

Reviews.

COMPARATIVE ANATOMY AND PHYSIOLOGY. By F. JEFFREY BELL, M.A. (London: Cassell and Co. 1885.)

To the student who desires a succinct and trustworthy description of comparative anatomy and physiology, as these are understood nowadays, and in a moderate compass, nothing better could be recommended than the excellent handbook written by Mr. Bell, and published by the enterprising firm of Cassell and Co. The importance of a knowledge of zoology must be greater to the veterinary surgeon than to the medical man who has to do only with his own species; and therefore the

subject of the comparative anatomy and physiology of all animals should form part of the study of the former, as it does that of the latter.

The plan of Mr. Jeffrey's book appears to us to be good, and the one most likely to render the information he offers interesting and readily instructive. Beginning with the *Amœba*, he goes on to describe the general structure of animals, and then takes up the subject of organs, which indeed forms the chief and most valuable feature of the work, the details of structure and function being worked out clearly and satisfactorily; while the comparisons of the simple with the more complicated organs are aptly rendered and clearly demonstrated. This fascinating study should have great attractions for the veterinary surgeon and student, and the help this manual should give to the beginner can scarcely be over-estimated. It is written for students of medicine, and therefore should be acceptable to veterinary students. It contains two hundred and twenty-nine wood-cuts, is neat and handy, and is published at the low price of seven shillings and sixpence.

THE EQUINE HOSPITAL PRESCRIBER. By J. B. and A. GRESSWELL. (London: Bailliere, Tindall, and Cox. 1886.)

This little book, consisting of seventy-six pages, has been, we are informed, written expressly for the use of veterinary practitioners and students, in order to supply a want which the authors believe to exist. They also remark that the large London hospitals are provided with pharmacopœias of their own, which prove to be of great value. In order to extend the usefulness of their little "Prescriber," they have departed from the plan of such works, in arranging their formulæ under the special diseases for which they are drawn up. By this deviation the book is rendered more handy for reference. As the value of the prescriptions have nearly all been proved by extensive use, and as they are numerous and varied, and written in plain English, they should be very acceptable to veterinary surgeons who are in engrossing practice, and require the information and references to be in concise form and clear language.

STRANGEWAYS' VETERINARY ANATOMY. Edited by PROFESSOR VAUGHAN, F.R.C.V.S. Third Edition. (Edinburgh: Bell and Bradfute. 1886.)

The present edition of this excellent students' manual is much improved by new cuts borrowed from various English and German works; and the careful way in which the editor has performed his task renders the book one of the best of its kind in the English language.

HORSES AND STABLES. By LIEUT.-GENERAL SIR F. FITZWYGRAM, Bart. Third Edition. (London: Longmans, Green and Co. 1886.)

The revision of this work having been largely made by Inspecting Veterinary Surgeon Walters, many of the erroneous statements and strange notions which appeared in the former editions are absent in this. The really useful chapters to horsemen, and perhaps to veterinary students, are those relating to stables, stable management, feeding, and forage. The chapters on the treatment of diseases and accidents are too abstruse and embarrassing to be of any use to amateurs, unless it

is believed, as the author must surely do, but which he will not often find to be the case, that horsemen are thoroughly well up in anatomy, physiology, pathology, clinical medicine, operative surgery, and all the skill and manual dexterity required to play the part of a veterinary surgeon. If these chapters had been omitted and their place taken by others on nursing sick horses, in which the amateur could be of real use, the book would have been greatly improved, and a service would have been rendered to keepers of horses. If an amateur attempted to treat his horses according to the directions given, he would soon find himself a confirmed pedestrian; while as to bleeding, neurotomising, trephining, and other operations, we would like to be present when the amateur tried his hand at them. Veterinary surgery is not learned by reading books, but by a serious course of study, which embraces a somewhat wide range of subjects and requires much time; while constant practice is needed to acquire dexterity and confidence. The public should know this; but books of this kind are likely to make them believe that horse-doctoring is easy, and that but little skill is required in the veterinary surgeon. We notice that Glanders and Farcy are stated to be not really developed by infection or contagion. We had hoped such a notion was exploded. There are still errors in the work, but no doubt in the course of time they will disappear. One of these should be particularly noted, as it has already led to several serious falls and broken limbs among troop horses. It is that connected with stable floors, in which longitudinal grooves, extending from the manger to the heel drain, are recommended, instead of diagonal from the sides towards the centre of the stall.

AN EPITOME OF VETERINARY MATERIA MEDICA AND THERAPEUTICS. By T. J. SYMONDS, M.R.C.V.S., F.L.S., and J. MILLS, M.R.C.V.S. (Madras: Madras School Book and Vernacular Literature Society's Depot. 1885.)

WEIGHTS AND MEASURES, POSOLOGICAL TABLE, AND INDEX TO VETERINARY MEDICINES. By the Same. (Lawrence Asylum Press. 1885.)

These two works, published by the Madras Government, are the joint production of two officers of the Army Veterinary Department, one of whom is the lecturer on Zoology and Veterinary Medicine and Surgery at the Agricultural College, Saidapet, in that Presidency. The first of the works forms one of the Madras Science Primers, and is published by order of the Director of Public Instruction. It is intended as an introduction to the systematic study of veterinary medicine, and has been compiled for the use of the students of the above-mentioned college. Though in its compilation recourse has been had to various standard works with which the veterinary profession is acquainted in this country, yet the addition of drugs indigenous to India renders the work more valuable than any of these to the student and practitioner in that country. The arrangement is convenient, and, so far as we can see, there are very few errors.

The second work appears to be merely a separate reprint of the concluding portion of the first, presented in this form for convenience.

The two authors deserve much praise for the excellent manner in which they have accomplished their task. The Pharmacopœia is a model of terseness and completeness; the Appendix, containing the natural orders of medicinal plants, with their essential characters, and other information, forming a most useful complement to the materia medica and therapeutic section. The work can be highly commended, and as strongly recommended to members of the profession, not only in India, but in this country.

Proceedings of Veterinary Medical Societies, &c.

ROYAL COLLEGE OF VETERINARY SURGEONS.

QUARTERLY MEETING OF COUNCIL, HELD OCTOBER 1ST, 1886.

The President, Dr. Fleming, in the chair.

Present.—Professors Axe, Brown, Duguid, Pritchard, Robertson, Walley Wragg; Messrs. J. D. Barford, Cartledge, Carter, Cartwright, J. R. Cox Dray; Dr. Fleming; Messrs. Greaves, Harpley, Mulvey, Perrins, H. L Simpson, J. F. Simpson, Simcocks, Peter Taylor, Whittle, Woods, and the Secretary.

The SECRETARY read the notice convening the meeting.

The minutes of the previous meeting were confirmed.

The SECRETARY said he had received letters from Professors McCall and Williams, and Messrs. Santy and Campbell regretting their inability to attend the meeting.

Presentations.

Several drawings were presented to the Museum of the College by Professor Axe, being part of his promised contribution of £10. The Museum was also presented with specimens of calculi by Mr. Naylor, of Wakefield.

The following presentations were made to the Library:—

"M. Pasteur and his Work," by Dr. George Fleming (President); "A Manual of Chemistry," by Dr. Dupré; a pamphlet, "Revolution in Horse-shoeing," by Mr. C. F. Jeffery; "Handbook on Sandcrack," by Mr. W. South; a copy of Mr. J. H. Steele's work, "Disease among Transport Mules in British Burma."

On the motion of Mr. DRAY, seconded by Mr. PETER TAYLOR, a hearty vote of thanks was accorded to the donors of presentations.

A diploma was signed in favour of Mr. William W. Hill, holder of the Highland and Agricultural Society's certificate.

Correspondence.

The SECRETARY read a letter from Mr. John Steele on behalf of the Bombay Veterinary College, requesting that that body might be recognised as a veterinary college.

On the proposal of Professor AXE, the Secretary was instructed to write for an official prospectus, in order that the Council might satisfy themselves as to the constitution of the college and its curriculum.

The SECRETARY said that according to instructions he had on the 4th of August last written a letter to Her Majesty's Lords of the Treasury with regard to County Court Fees, but had not yet received a reply.

At the suggestion of Mr. PERRINS, the Secretary was instructed to write a second letter requesting a reply to the letter of the 4th of August.

The SECRETARY read a letter from Mr. A. C. Robertson, member of the College, with regard to his brother, Mr. W. C. Robertson, an unsuccessful applicant for registration under the Veterinary Surgeons Act, asking the Council to reconsider its decision.

Mr. H. L. SIMPSON moved that the further consideration of the case be referred to the Registration Committee.

Mr. PETER TAYLOR seconded the motion, which was agreed to.

The SECRETARY read a letter from Professor Walley suggesting the following additional qualifications exempting students from examination in general education:—

(1) Society of Science, Letters, and Art of London, 1st Class Certificate of Merit.

(2) Education Board of Ireland, Senior Grade.

(3) Apothecaries' Hall, Ireland.

(4) Military, Naval, Civil Service, Marine, and Divinity—any part of the United Kingdom, provided the subjects mentioned in the certificates are those laid down by the Royal College, and of equal value.

Professor WALLEY said that the matter was a most important one, and urged upon the Council the necessity of adopting his recommendations.

Professor AXE moved, as an amendment, that the entire question be referred to a Committee for consideration, in order to inquire into the value of the standards of examinations of different bodies which were to be recognised by the Royal College of Veterinary Surgeons.

The PRESIDENT thought it would be better to accept Professor Walley's motion, and take into consideration, in the bye-laws or elsewhere, the value to be placed upon the different certificates. If the Royal College recognised other standards as co-equal with its own, he did not see what more was required.

Professor BROWN said he thought the matter should not stand over any longer, and he would therefore second Professor Walley's motion.

The amendment being put to the meeting, eight hands were held up in its favour.

The original motion was then put, eight also voting for it, but the President, giving his casting vote in its favour, declared the motion to be carried.

The Council has been informed that the Society for the Prevention of Cruelty to Animals has decided not to proceed further in the intended prosecution of the individual in respect of whose case the Society obtained permission to prosecute from the Council of the Royal College of Veterinary Surgeons, in accordance with the Veterinary Medical Act.

Report of the Finance Committee.

Mr. DRAY moved that the Report of the Finance Committee be received, and if sanctioned by the Council, be adopted, and cheques drawn for the liabilities. He had pleasure in stating to the Council that through the exertions of Mr. Cartwright the following subscriptions had been promised to the College Building Fund:—Mr. C. Sheather £25, Mr. G. Williams £10, and Mr. W. Wilson £2 2s. The probable amount the College would have further to pay was from £300 to £500. He hoped further subscriptions from the profession would be obtained in order to get the building out of debt.

Professor WALLEY seconded the motion for the adoption of the financial statement, which was agreed to.

Report from the Court of Examiners.

The SECRETARY read the Report of the Examinations.

Mr. J. ROALFE COX said that in the ordinary course the Council would not meet again until January next, and as it was not unlikely for the Council and

President to receive an appeal to grant an examination of students at Christmas next, he therefore ventured to call the attention of the Council to the subject. They all knew that there were two examinations held in the year—one in the month of April, and the other in July. It had already begun to be felt by the principals of schools and by students that those two periods of April and July were either inconveniently close or the subsequent interval inconveniently long. As it was extremely probable that an application would be made to the Council that an examination might be held at Christmas, he thought the question ought to be considered as to whether the President should be empowered to grant it or not. He was inclined to think that most of the older students would feel it a hardship if they had not the opportunity of going up for examination at Christmas.

The PRESIDENT reminded the Council that it was decided some time ago that either during the present or following year a change should take place in the time of the examinations. Two examinations would be held in a year—one in April, and the other at any time that might be decided upon by the Council. Students who had entered the schools before the passing of the regulations of the College were entitled to examination in January if necessary, but there must be at least nine candidates.

On the suggestion of Professor WALLEY, the Secretary was directed to ascertain when the regulations were passed and should come into operation, and to inform the principals of the schools.

Report from the Examiners for the Fellowship Degree.

The SECRETARY informed the Council that an examination for the above degree was held in Edinburgh on July 2nd, when the following gentlemen were admitted Fellows of the Royal College of Veterinary Surgeons:—Mr. Finlay Dun, Member of the Board of Examiners; Mr. John Bennett, Maybole, Ayrshire; Mr. Thomas H. Simcocks, Drogheda; Mr. Andrew Smith, Principal Toronto Veterinary College; Mr. John Freeman, Dublin; Mr. Benjamin Smith, Barnsley, Yorkshire; and Mr. James Storrar, Chester.

Obituary.

The SECRETARY read the obituary list.

The Secretary was instructed to send a letter of condolence to the widow of the late Mr. Samson Gamgee, expressing the extreme sorrow which the corporate body felt at the death of her husband.

Professor WALLEY also thought that a letter of condolence should be sent to Mr. Gamgee's father in Edinburgh.

The proposal was unanimously consented to.

Complaints under the Veterinary Surgeons Act.

Mr. WHITTLE said that it had come to his knowledge that several men had been infringing the Veterinary Surgeons Act. Those men had been warned once or twice. It had been proposed and seconded at a prior date that those cases should be left for investigation by the Registration Committee, which was to have power to act without referring to the Council with regard to prosecution. He presumed the same course was to be adopted in the future.

The proposal was agreed to.

Honorary Associate.

The PRESIDENT proposed as Honorary Associate the name of Mr. D. E. Salmon, of Washington, a very distinguished man.

Professor BROWN seconded the proposal, which was unanimously agreed to.

The PRESIDENT called the attention of the Council to the design of a stained-glass window, to be placed in the Council-room, the gift of the Cen-

tral Veterinary Medical Association. It was for the Council to decide whether the window should be put in, or whether the matter be left to the Committee to decide with regard to the fitness of the design.

Professor PRITCHARD proposed that the design then before the Council be accepted.

Professor WALLEY seconded the motion, which was unanimously agreed to.

On the motion of the PRESIDENT, a grateful vote of thanks was accorded to the Central Veterinary Medical Society.

SPECIAL MEETING.

Mr. J. ROALFE COX then moved the following notice of motion :—"To alter Bye-law No. 49 so that it be read as follows : 'In the month of July, annually, the Council shall appoint a Special Court of Examiners, consisting of not less than three members, for the purpose of examining candidates for the degree of Fellow.' 'An examination for the degree of Fellow shall be held in London in January, April, July, and October, in each year, provided that the names of at least four candidates shall have been entered for that purpose, and the fees paid to the Secretary of the College prior to the examination taking place. And a like examination shall be held in Edinburgh twice in each year during the months fixed for holding the General Professional Examinations.'"

Professor WALLEY seconded the motion.

The motion was put and agreed to.

The PRESIDENT observed that there must be a meeting within a fortnight to confirm the bye-law.

Professor AXE then brought forward the following motion for the consideration of the Council :—"That any member of the Royal College of Veterinary Surgeons having held the office of President or Vice-President ot the College for two years shall not be eligible for re-election—in the case of President—until the expiration of ten years ; and in the case of Vice-President until the expiration of five years ; and that no member of the profession shall be eligible for either appointment unless he has been at least ten years in active practice." The object, he said, in bringing the motion before the meeting was in order that a less partial and more widely distributed enjoyment might be participated in by the members of the Council in the official honours of the College.

Mr. PERRINS seconded the motion. Several members of the Council having somewhat unfavourably criticised the motion,

Professor AXE said, that having taken the sense of the meeting he should be glad to withdraw the motion.

A vote of thanks, proposed by Mr. DRAY, to the President for his conduct in the chair, terminated the business of the meeting.

SPECIAL MEETING OF COUNCIL, HELD OCTOBER 15TH, 1886.

DR. FLEMING, President, in the chair.

Present: Dr. Fleming, Sir F. Fitzwygram ; Messrs. Mulvey, Wragg, J. F. Simpson, Whittle, H. L. Simpson, J. Roalfe Cox, Greaves, Dray, Barford, and the Secretary.

The SECRETARY read the notice convening the meeting.

The minutes of the last meeting were read and confirmed.

The SECRETARY announced that letters expressing regret at their inability to be present at the meeting had been received from Messrs. J. Carter, McCall, Cartwright, and Taylor.

Presentation to the Library.

The SECRETARY announced that a copy of the "Illustrated Handbook of Victoria, Australia," had been presented to the College on behalf of the Royal

Commission of Victoria by the President of the Commission, Mr. J. Bosisto, and the Secretary and Editor, Mr. James Thomson.

On the motion of Mr. DRAY, seconded by Mr. WRAGG, a vote of thanks was passed to the donors.

Honorary Associate's Diploma.

A diploma in favour of Mr. D. E. Salmon, of Washington, was duly signed.

Correspondence.

The SECRETARY stated that he had received a letter from Professor Walley, asking whether there was any limit as to the date of the certificates exempting candidates from the matriculation scheme of the College.

The PRESIDENT said there was no limit, provided the subjects mentioned in the certificates were those laid down by the Royal College, and of equal value.

Alteration of Bye-law.

The SECRETARY read the motion for alteration of a bye-law, proposed by Mr. Roalfe Cox, which was passed at the last meeting. The motion was as follows:—"In the month of July annually the Council shall appoint a special Court of Examiners, consisting of not less than three members, for the purpose of examining candidates for the degree of Fellow. An examination for the degree of Fellow shall be held in London in January, April, July, and October, in each year, provided that the names of at least four candidates shall have been entered for that purpose, and the fees paid to the Secretary of the College prior to the examination taking place, and a like examination shall be held in Edinburgh twice in each year during the months fixed for holding the general professional examinations."

Mr. H. L. SIMPSON asked what was the net profit to the College from each examination after paying the Examiners.

The PRESIDENT said that if four gentlemen came up for examination the fees of fifteen guineas each would amount to sixty guineas, out of which the four Examiners were paid four guineas each.

The SECRETARY said three guineas were paid also to a member of the College of Preceptors.

The PRESIDENT said that would not be paid in future. Four candidates was the smallest number that could be examined.

The seal of the College was affixed to the new bye-law, which was duly signed.

The PRESIDENT stated that this concluded the business of the Special Meeting, but he thought that reports of what the committees had done should be entered in the minutes of this meeting.

Registration Committee.

The SECRETARY said that ten cases had been brought before the Registration Committee, two of which had been left for the solicitor to deal with. These were, first, the case of Mr. Cooper, a registered practitioner, who had been using the College arms and styling himself a member; and, next, that of Mr. Allen, *alias* Turner, who had already been fined, but who still continued to make use of the title and to advertise himself, and was therefore to be prosecuted if he could be got hold of.

House Committee.

The House Committee had accepted the tender for lights for the Board Room, the amount being £26 7s. 6d. Permission was given for the top of the centre window in the Board Room to be closed permanently (according to

Mr. Wragg's suggestion), should the Central Veterinary Medical Society require it, for the purpose of erecting their Memorial Window. It had also been recommended that a wire screen should be placed over the windows at the back (stained windows) of the room, the meshes to be as wide as possible so as not to interfere with the light.

On the motion of Mr. WHITTLE, seconded by Mr. DRAY, the proposals of the Committee were adopted.

County Court Fees.

The SECRETARY announced that he had again written to the Lords of the Treasury about County Court Fees, but had received no reply.

He was instrueted, if necessary, to wait upon the Lords of the Treasury about this matter.

A vote of thanks to the Chairman terminated the proceedings.

ROYAL VETERINARY COLLEGE.

OPENING OF THE SESSION, OCTOBER 1ST, 1886.

LORD RIBBLESDALE presided at the opening of the winter session of the Royal Veterinary College, Camden Town, on October 1st, the inaugural address being delivered by the Principal, Professor Robertson.

The PRINCIPAL, addressing the students, said: I have pleasure in introducing to you the Chairman of to-day, Lord Ribblesdale, one of the Governors of the College. (Cheers.)

The CHAIRMAN said: Mr. Principal and Gentlemen,—Before I call upon Professor Robertson to deliver his lecture, I must thank you for the very hearty and kind reception you have given me. I beg now to call upon Principal Robertson to deliver the prefatory lecture.

The PRINCIPAL, who was received with loud applause, spoke as follows:—

MY LORDS AND GENTLEMEN,—I am well aware that opening lectures or inaugural addresses are regarded with rather different opinions and feelings by different individuals. By many they have not merely been looked upon as a worn-out relic of the past, but actually condemned as serving no useful purpose, as consuming time which might be more profitably employed. By others, amongst whom I would desire to take my place, they would still be retained, not merely in veneration of the past, but as affording an opportunity for the exercise of the courtesies and amenities of associated life, as well as for the inculcation of sage advice and direction, which, although old and often repeated, is not the less valuable and needed for success in life in the present than it has been in the past.

1. To you, our friends who are either members of the profession, and who have a varied remembrance of student life, and to those who, although not actually co-workers in the department of animal medicine, have yet shown your interest in it and sympathy with us teachers and workers, I, on behalf of this institution, give you a hearty welcome. Sympathy is dear to all men, and the greater number of us do not like to feel that we are isolated, but rather that we are an essential part of the great whole; that in truth there is something in each which is of interest to, and goes far to form part of, the whole. Many, I doubt not, sitting here to-day cannot help recalling to their recollection forms and voices, now departed, the possessors of which, while they lived, gave reality to the scene around by their instruction and guidance. Such memories are good for you as intensifying previous impressions, and by asking you the question if you have acted up to instruction given and to promises apparent, they are good for us who fill their places. For myself, I fancy that the shades of many of these, peering from the unseen, seem to upbraid, not with the view of punishment, rather by way of encouragement. They seem to say, Recollect

what we did with our slender opportunities, and do not forget that your position as respects advantages is infinitely superior. Are your results in keeping with these? This applies both to teacher and scholar.

2. To you, students proper of veterinary medicine, whether for the first time or not making your acquaintance with actual scholastic work, I would desire to remind you of a few things, and to impress upon you the importance of a few rules of conduct. Your presence here to-day is so far an assurance that you have made a choice, and, I trust, a deliberate one, of this particular department of medical study as your life-work. Presupposing that you possess the requisite qualifications for this pursuit, the chief and most important of which is a selective affinity or fondness for it in its several departments and details, you are following a correct course in appearing here to-day. The next essentials towards rendering you properly equipped experts in the healing art as applied to those animals which will come under your care may be expected to be found, or are to be cultivated within, the walls of this institution. These are—(1) Such as pertain to *time, place, and opportunity* for acquiring a competent understanding of those different branches of knowledge which, in the meantime, are considered needful preparatory to entering upon your professional work, and *instructors* or guides qualified, not merely to teach in the limited sense of the term, but who are imbued even more largely with the faculty of creating in you an interest and enthusiasm in the several studies in which you engage, and thus of implanting in all an earnest desire after truth, and a determination and ability to educate themselves. (2) The cultivation of certain *personal qualifications* by which you will be able to utilise and appreciate the former.

Before, however, I make any statement as to the extent to which we propose to be able to fulfil the former of these, or of the necessity which there exists that you should possess the latter, I would desire very shortly to remind you of the altered position which you of to-day occupy, compared with that in which your fathers stood.

The entire science of biology, including physiology and pathology, as well in their simpler and purer developments as in their practical applications to medicine, curative and preventive, has during the present generation undergone wonderful changes. What little has been accomplished in hygiene and preventive medicine of man or of animals, has been chiefly the result of our better acquaintance with fungoid parasites, their genesis and the means which may be successfully employed to check their growth. The discoveries belonging to this department of knowledge may justly be regarded as the most important and far-reaching which have blessed humanity since the days of Galen.

The idea of life as related to form and organs in all living beings has for long been found inadequate to explain phenomena always recurring and presented to our senses. Physiology now takes into consideration the actions of living beings on their surroundings, and the influence of surroundings on living beings.

The mere knowledge of structure, form, and movements of living beings, tell us only a part, and that a superficial part, of what we want to know. Much of this form and movement of animals, formerly regarded as expressing what we knew of life, we now believe to be essentially dependent on changes of an intimate molecular character constantly occurring in living bodies. That every living body dies daily, in the sense that there is a continuous change, oxidation, or combustion, resulting in the liberation of energy, and the collecting of other materials less prone to change—that life is in its every act really the outcome of death — this is more truly the ground upon which the physiology of the present seems reposing. Further, we seem now tolerably satisfied that what we call protoplasm, the molecular

basis of living being, is not one, definite or unvarying in character, any more than it is possessed of stability in situations wherever it is found, not even in the same animal in its different organs, or at different times; that whatever we may recognise or regard as the fundamental properties of this material, that over and above the underlying essentials it is subject to, and exhibits many and varied modifications; that it, changes in one form may result in heat, and in another motion with heat, with, it may be, change of form.

From this knowledge and these ideas our mode of viewing "life" has been materially altered. Few of us would now be disposed to regard this or speak of it, as formerly, by the terms of "animal or vital spirits," as something intangible added to the material. The position is one rather of expectancy and desire, so that, whether recognising such as a property of protoplasm, a special condition of matter, or an expression of internal molecular movement, we are disposed to wish and to wait for further knowledge, while in our present uncertainty we would not forget to remember that "it is positive knowledge to know that a thing cannot be known." Thus far would I lead you in matters biological as indicating the ground which we now occupy.

In matters more directly of practical application I may say that never since medicine emerged from the darkness of the Middle Ages has it in every department presented itself so clearly, I may say, so ostentatiously, before the eyes of men, not merely as a healing art, but, what is far better, as the repressor and destroyer of those agencies which by their influences on living beings induce the disturbance we know as disease. Neither the discovery of the circulation of the blood nor ot vaccination were attended with results so notable and far-reaching as the actual and prospective benefits resulting from experimental investigation within the last quarter of a century.

In both human and veterinary medicine the elaboration of the germ theory of Pasteur, in its earlier triumphs in the field of surgery initiated by Lister, is likely to be surpassed by what we may reasonably expect will yet be achieved in the domain of medicine. The probable influence upon the spread of contagious and specific diseases amongst men and animals has been, and is likely further to be, influenced by the following out of the idea that each specific disease has a specific virus, while our direct and immediate control over these is materially strengthened if it be further proved that for each specific poison a special antidote or vaccine exists.

In preventive medicine as applied to animals, the recognition of the specifidity of many of those diseases which have attracted attention by their destructiveness and their power of impoverishing communities and nations, has given us a solidity and basis in dealing with them which was utterly unattainable while they continued to be regarded as the result of varying influences, sanitary or dietetic. What aimless wandering and uncertainty as regards results must ever follow any measures adopted with a view either of circumscribing or eradicating such diseases as Glanders or Epizootic Pleuro-pneumonia, while these continue to be regarded as being capable of development by any agencies apart from previously existing Glanders or Pleuro-pneumonia!

The doctrine of the production of a protective material obtained by the cultivation or modification of the specific virus of these diseases, although not yet undoubtedly demonstrated in all its fulness, clearly indicates, from what has already been brought out, that a new field in pathological inquiry has been opened up, and that results of marvellous and far-reaching influence, almost beyond our conception, seem shadowed more or less distinctly around us. This idea seems destined to demand that the entire subject of pathology shall be recast, or, at least, viewed from an entirely different standpoint. There is only wanting at the present time a few more facts, the repetition with

similar results of a few more experiments, and the whole of the ground recently travelled over will need to be reviewed with the object of recasting most of our older notions; while the theory of development in living forms, when studied in the lowest as in the highest, may yet explain many curious phenomena in the matter of action of the poisons of these specific diseases, seeing that the same facts relating to growth and development are probably applicable to the lowest as to the highest forms.

From information, meagre though it be, which we now possess, obtained from experimental inquiry and inductive reasoning, very different views are being entertained regarding the whole subject of disease. Placed upon a broader and more comprehensive basis, disease is now being regarded as something more than mere discomfort and disturbance attributable to immediately existing extrinsic agencies. In its varied disturbances the animal body, both in its internal, visceral and its locomotory organs, "manifests the impress of its inheritance" or the results of its embryonic development. With a faint and fitful light there seems dawning upon us the idea that possibly disease is not merely a morbid change the effect of casual influences, "but to a certain extent the undoing of evolution, a species of degradation or reversion being entailed thereby, or, in other words, a dissolution or return to more primitive and lower forms of life."

Practically we are now aware that many diseases of animals which are as yet beyond the sphere of curative medicine are capable of prevention.

I know, gentlemen, that the study of these matters in their highest point of view as affording us an opportunity of insight into some of the wondrous processes of the universe, is not likely to bulk so largely in the consideration of those preparing for qualification in a particular department of life's work, as their ascertained value to aid you in the more practical studies which must engage your attention. There is no reasonable doubt in the minds of intelligent and thoughtful men, that the study of medicine both as a science and an art, is essentially one, whether the subjects to which it be applied be men, or those we ordinarily designate the inferior animals. If disease is to be treated, or its causes investigated with a view to prevention, work of essentially similar nature requires to be done, whether men or cattle are the subjects of our operation. The structure and functions of the animal bodies which demand our attention are very similar to those of human beings. The medicines which act on the one operate in the majority of instances not greatly different in the other, while the laws which regulate epidemics or endemics are such as we find operating in the distribution of epizootics and enzootics. I am well aware that the idea is commonly entertained that the veterinary surgeon does not for his special work require to be so highly trained and educated an individual as his *confrère*, the practitioner of human medicine. In the abstract this is untrue, and is only tolerated from the justly acknowledged value of human life above that of our common animals. Without, however, entering upon the discussion as to the importance of the veterinary profession, and the value, especially to a country such as ours, of a properly trained body of animal physicians, into whose hands may be committed the medical, surgical, and hygienic treatment of our live stock, representing so much of the wealth, the prosperity, and comfort of the nation, I can only say that it is long since I came to the conclusion that the practitioner of veterinary medicine ought to receive the same careful preparation for the exercise of his particular work as is bestowed upon his co-worker in the field of human medicine. If a knowledge of botany, geology, parasitology and other cognate sciences are regarded as expedient or needful for him who conserves our own health, they are at least equally required for those who profess to take charge of that of our animals; while in the diagnosis of disease—a more difficult work in animals than in man—the veterinarian requires more

fully to avail himself of all the aids which science has placed in his hand, such as those obtained by skilful use of the thermometer, the microscope, stethoscope, etc., and to accustom himself to careful and repeated examination of animals in both health and disease. As he must depend on objective phenomena chiefly for his information, the habit of rapid and accurate observation, of delicate manipulation of parts, together with quick appreciation of deviation from health standard, is essentially necessary. For all this, the employment of the means for collecting information, the opportunities afforded by a varied and extensive clinic, are more needful in his case than in that of his fellow-practitioner attending on living men.

Considering all these things, the present standpoint of medical science and art, the worthiness of the work of the department in which we are engaged, and its large requirements; and presupposing what I have already taken for granted, viz., the fondness or aptitude for it, and without which I dare not advise you to proceed, I will now very briefly notice some prominent requirements needful to secure the ends which your enrolment here contemplates.

The first three, *time*, *place* or *opportunity*, and *instructors*, we propose to supply you with; the last three, *earnest work*, *observation* and *reflection*, and a *proper observance of the social relations*, you must provide for yourselves.

A. *Time.*—The period of time required for you to devote your energies to acquiring the determined amount of knowledge you are expected to possess ere entering upon the arena of life is none to great, but may, with well-directed and diligent efforts, be found sufficient to secure what is demanded.

Thanks to the wise discernment of our teaching schools, supported by the corporate body, this has of late years been considerably extended, and probably under certain conditions and with some changes may require further extension. Few of you are now, unless in exceptional circumstances, under the necessity of making your collegiate work of secondary moment, snatched in fact from that employed in what was more imperative and of immediate importance. Frequently have I asked myself the question, Do students fully appreciate this boon? In many instances I fear they do not. No one can perform impossibilities. The amount of work now demanded of the student necessitates that his whole time from first to last shall be devoted to these special subjects, without the distracting and wasting influences of separate engagements. This, I am glad to say, is now secured to you through the settled purpose, directly or indirectly, of your parents or guardians. Recollect, then, this is a sacred trust, which, if well employed, is the first needful step towards the attainment of your contemplated purpose; but do not forget your responsibility to those who have given it to you, and that if squandered recovery is impossible, as it is the surest means of placing you on the high road to blasted hopes and disappointed expectations.

(*To be continued.*)

SCOTTISH METROPOLITAN VETERINARY MEDICAL SOCIETY.

THE quarterly meeting of this Society was recently held in the London Hotel, Edinburgh. There was a small attendance, and in the absence of Principal Williams, Mr. Fairbairn, Cupar, Fife, occupied the chair.

A motion by Professor Walley, with regard to the reporting of the Society's meetings, after some discussion. was withdrawn. A lengthy discussion followed on the paper read by Mr. Robertson before the recent meeting of the National Veterinary Association on "Hæmorrhage." Several cases in point were related, and the means of prevention and cure were discussed.

Principal WALLEY thereafter referred at considerable length to several important technical subjects as follows:

THE USE OF COCAINE.—The value of cocaine as a local anæsthetic is, I presume, a matter of common knowledge; by its aid we can, and do, anæsthetise mucous membrane, skin, and other structures, thereby facilitating the removal of abnormal growths and the performance of operations. It is not, however, in this connection that I wish to direct your attention to this agent to-day, but rather to its ophthalmological use, especially in the treatment of corneal ulceration.

The series of cases (all in dogs) in which I have used it number six, and in each case the ulceration appeared to have had an idiopathic origin. I should not, indeed, apply it in the case of ulceration having a specific origin, or of a purulent type. In my cases the edges of the ulcers were clean cut, and in several instances were associated with distinct capillary injection of the cornea.

In only two out of the six cases—and these the most simple in character—am I warranted in saying that this agent exercised a curative effect; in fact, after giving it a fair trial, I had to resort to the usual methods of treatment in four of the cases. The particulars of one of the two successful cases are, I think, worth detailing. The subject was a collie dog; the ulcer was situated in the centre of the cornea of one eye, but in the course of a few days the other became affected also. The cure here was rapid and complete for a time at least, but recurrence took place in the course of a few weeks, on which occasion the reapplication of the agent was as successful in effecting a cure as on that of the first attack. That cocaine is a useful adjuvant in the treatment of corneal ulcerations, I am quite prepared to admit, and should advise its use for the purpose of relieving pain, and thus, indirectly, favouring the production of a cure; but its power as a curative agent, pure and simple, is limited. In one case of nerve irritation in a dog, marked by inability to bear light, but in which no organic change could be detected, I found the agent of great benefit.

PHYSOSTIGMINE IN INTESTINAL AFFECTIONS.—The use of this agent has been mainly confined to the treatment of Colic Paresis, produced by indigestion, and of Colic Tympany in the horse; but, personally, I have for long thought that it might with advantage be used in retention of urine due to cystic paresis, in partial paralysis of the œsophagus, and in cases of displacement of the colon. In the two first-mentioned conditions I have not had an opportunity of applying it. In the last I have, and, as I shall presently show, with striking results.

In Colic impaction I have tried this agent thrice, and in each case after adopting the usual method of treatment. In the first of the three cases the pain ceased immediately after the administration of the physostigmine; but this might well have been a mere coincidence. In the other two cases no change of any importance was observed, and in none of the three did the bowels react any more rapidly or more powerfully than under the ordinary method of treatment.

In reference to displacement, or partial torsion, of the colon, I may observe that it had occurred to my mind that there was just a possibility of the powerful contractions of the bowel, excited by physostigmine, producing reduction of the lesion; and, I believe, from my experience of its effects in one case, that it tends to do so. The case in which I applied the agent, in the form of eserine sulphate, was one in which, from the symptoms presented and from the conditions detected by rectal exploration, I am satisfied that the sigmoid flexure of the colon was thrown over to the right lumbar region, but in which there was no vascular strangulation. The horse was ill for a period of six days, and it was not until the expiry of forty-eight

hours that I was enabled to satisfy myself as to the exact nature of the case with which I had to deal. Consequently, before eserine was used, a full dose of cathartic medicine had been administered, followed by opiates and several doses of oil. On the evening of the fifth day the involved intestine became markedly tympanitic, and, partly with the view of relieving tension and pain, and partly in the hope that in the removal of the gas the bowel would contract, and thus bring about reduction, I performed paracentesis intestinalis, with the result that a very large volume of gas, having an abominable odour, escaped, and the horse gained decided relief; but, the main condition persisting, the following morning I decided upon administering a dose of physostigmine. This was done, and in about thirty minutes reaction of the bowels took place, and after the expulsion of two or three hard masses, an enormous quantity of fæces in a semi-liquid condition was got rid of, and the enteric symptoms subsided, and did not return.

"One swallow," it is said, "does not make a summer," and I do not pretend for a moment to say that with eserine we can accomplish the reduction of every case of displaced or twisted colon; in this particular instance, I believe that reduction of the bowel was facilitated by my having previously placed it in a favourable state for reduction, by relieving the tympany. In complete torsion we would scarcely expect reduction from the use of this agent. I may remark, in conclusion, that my friend, Mr. F. W. Barling, of Ross, has lately given this agent a fair trial in several cases of Acute Indigestion in cattle, but without any favourable result.

INTESTINAL WORMS.—In bringing before your notice the subject of "The Development of the Intestinal Worms," I may remark that I have been induced to do so owing to the publication of a paper on "Equine Parasitology" by Professor Robertson—a paper read by him at the May meeting of the North of England Veterinary Medical Association—in which he makes several remarks relative to the development of intestinal parasites, with the truth of which my clinical and practical experience does not enable me to agree. These remarks, I may observe, would have been made by me at the last meeting of the North of England Association, but I was not able to arrive at the place of meeting in time to take part in the adjourned discussion on the subject. With your permission I will first notice certain remarks made by Professor Robertson in reference to the development of intestinal parasites, after which I will notice *seriatim* the points in which I materially dissent from the conclusions at which he has arrived.

Etiology and Development.—Professor Robertson says: 1. "Although much has to be learned on these points, we know sufficient to convince us of the *great probability* that no parasite originates from ova deposited by the perfect worm in the canal (intestinal) itself." "The ova, we are disposed to believe, must be discharged from the bowel, and, after undergoing metamorphosis outside the body of the former host, are again taken into the animal body destined to afford them a habitation where they may perfect their existence." Then speaking of *Ascaris megalocephalus*, Professor Robertson says: "The eggs are passed out with the fæces, and find their way into moist places. They possess great vital resistance, and produce, under favourable conditions, minute embryos in fourteen days. Having undergone certain developmental changes, they appear as free swimming larvæ or vermicalis, in which condition *it is probable* they are swallowed with water or fresh fodder. On reaching the stomach or intestinal canal of their host they grow rapidly, and reach their maturity in three weeks."

Speaking of *Strongylus armatus*, Professor Robertson says: "The formation of embryos in the egg *seems* to follow the extrusion of these from the parent worm and the host. The larvæ undergo similar changes to those of the ascarides, and ultimately gain access to the bodies of intermediary

bearers—as insects or their larvæ—and are taken into the stomach of the ultimate host in drinking-water. *They are more common in young than in old animals.*"

Speaking of *S. tetracanthus*, Professor Robertson says: "The ova are expelled from the intestines, and, after undergoing certain changes, are returned thence in much the same manner as in the case of the *S. armatus*." "From the intestine the immature worms bore their way into the walls of the tube, where they may be observed in great numbers, lying coiled in distinct cavities or sacs, inducing, through their presence, much irritation, leading to fatal congestions and inflammations." "In these capsular cavities they remain for a certain time, and undergo certain changes, after which they pass back (migrate) into the lumen of the intestine."

In re the *Oxyures curvula*, Professor Robertson says: "The ova, on extrusion from the parent worm and from the bowel, do *not necessarily require* any intermediary bearer in which to lodge previous to settling in the canal of the horse. It *seems probable* that they are conveyed to their host in a direct manner in food or water, or, it may be, in a dry state. In the stomach or small intestines the shell of the ovum is dissolved, and the embryo set free; in this situation *they seem* to abide for from two to four weeks until they reach sexual maturity, when they proceed onward to the large intestines, where they remain until discharged in the usual manner. *I believe I have seen* the eggs, which are frequently encrusted around the anus, and which have been detached from thence, and left adhering to prominent parts of the stable, through the rubbing of the affected part, being assiduously licked from the object to which they were adhering, and thus *anti-infection* may take place, *i.e.*, individual animals become reinfected by reingesting the ova from their own bowels."

The first two paragraphs I have quoted I will leave untouched until I have commented upon the others, and I do so because these paragraphs, it seems to me, contain the pith of the whole subject.

1. *In re A. megalocephalus*—(*a*) What proof have we that the development of this nematode takes place in the manner described? (*b*) Did Professor Robertson, or any other veterinary surgeon, ever see the young of this ascaris in the stomach of the horse? (*c*) Assuming that the development seems to take place in the manner described, what proof have we that the alimentary canal of the host was previously free from ascarides? In connection with these statements there is a saving clause, and that is in the words *probable* and *seems*.

2. *In re S. armatus.* Is it the fact that they are more common in young than in old animals? I have seen them located in the cavity of the vaginal tunics whilst castrating colts, and I have made *post-mortem* examinations of colts in the intestines of which I have found the entozoa in question, but I have never seen them in the mesenteric artery in any other than *old* asses or adult or old horses.

3. *In re S. tetracanthus.* I observe that Professor Robertson does not notice a very important and very constant lesion produced by this worm, viz., the *ulceration;* and while he directs attention to the fact that the worms may be found in great numbers in cavities or sacs, he says nothing as to the thousands of embryonic strongyles to be found there by the aid of the microscope. He further says that the worms remain within these sacs for a certain time and then pass back into the lumen of the intestines. What about the embryos which exist in hundreds? What becomes of them?

In re Oxyures curvula. Professor Robertson refers to the *probability* of their not requiring an intermediary bearer for conveyance to the alimentary canal of their host, and speaks of their abiding for several weeks in the stomach and small intestines, and he further states that he *believes* he has

seen animals assiduously licking the ova from stall posts, etc., against which the infected animal itself has been rubbing. (*a*) Has any person ever seen the young of the oxyures in the stomach or small intestines? (*b*) How is it possible for an infected animal to leave ova on a stall post, seeing that the ova are deposited in the perinæum—practically in a depression—and covered by the tail? (*c*) The mere fact of an animal licking a stall post proves nothing, as all horses will do this at times, especially when suffering from gastric or intestinal irritation

So much for Professor Robertson's remarks on these entozoa. Let us now see what practical experience teaches us. I have for long pointed out to my class that I did not believe an intermediary bearer was required for the development of certain intestinal worms—nor, indeed, for the most important of the bronchial parasites—and I have directed attention to one fact, viz., that horses the subject of ascarides, and of oxyures, were generally gross-feeding animals, devouring, whenever opportunity offered, their bedding, and even their own fæces ; and, more than this, that only certain animals in a stud were infested with these worms. Thus I have agreed with Professor Robertson "*that many horses* (he confines his remarks to the oxyures) *re-infest themselves.*" But in recent years it has been my lot to have under my charge studs of horses, the members of which are bedded with sawdust or peat-moss, and never see a green blade from one year's end to another, nor do they even get long hay, and yet a certain percentage of them (always the same horses) suffer from the ravages of ascarides or oxyures year after year, and in reference to *S. tetracanthus* I may say in all the cases that have come under my notice, I have found that even when confined strictly to dry feeding year after year, these worms could not be got rid of, and eventually proved to be the cause of death of their host. Two striking examples of this fact I may quote. The first was that of a horse belonging to my preceptor, Mr. Kettle, of Market Drayton, at the time I was a pupil with him. The second was that of a horse belonging to the Corporation of Birmingham, and the particulars of whose case were communicated to me by Mr. Malcolm. The former animal lived for many months, presenting the signs of intestinal (verminous) irritation and ulceration, and all the while was kept on dry food. The latter animal had not tasted green food except vetches the autumn preceding his death (he died in June) for five years, nor had Mr. Malcolm ever met with the *S. tetracanthus* in any other horse belonging to the same stud, either during life or after death. Moreover, in how many instances do we see ascarides in young puppies that have never tasted other food that that derived from their mothers ; and if gastro-intestinal worms only live for a short period in the alimentary tract of their hosts, how are we to account for the important organic changes seen in the stomach of the horse produced by the *Spiroptera megastenia*, and in the cæcum and colon produced by the *Strongylus tetracanthus?*

Twenty-three years ago I forwarded from Wales the stomach of a two-year-old colt, which had undergone marvellous organic changes, to the late Professor Varnell, of the Royal Veterinary College, London. In that case the changes were such as to convince the most ignorant that many months had been occupied in their production, and in that instance embryonic entozoa were found embedded in tumours in the structure of the organ, while half, and fully-developed worms were found protruding into its cavity, and lying between its coats.

In the case of the *Strongylus filaria* or *S. micrurus*, the *lung worm* of the lamb and the calf respectively, are we necessarily to believe that no intra-bronchial development takes place? I do not. On the contrary, I believe that development goes on, in some cases at least, in the air-cells and alveoli for an indefinite period, and for several generations, *i.e.*, if the host survives.

If there is one subject more than another which requires elucidation, it is the subject of the development of the nematoid entozoa of the domestic animals. Even the greatest of our helminthologists of the present or past generation (the late Dr. Cobbold) was sometimes utterly "at sea" in reference to the subject he taught; and when, many years ago, I ventured to suggest to him that the parasite he had described in Professor Williams's first edition of "Veterinary Surgery" as the *Trichinema arcuata* was nothing more nor less than the embryo of a strongylus, I met with such a reception that I never again troubled him with microscopic or any other parasites; and it is only as a matter of common honesty and justice that I controvert in this place a statement made by Professor Robertson, in an obituary notice of the late Dr. Cobbold, to the effect that, "To Dr. Cobbold belonged the credit of having introduced the study of helminthology into the curriculum of the (London) College." Years before Dr. Cobbold was connected officially with the College, Professor Simonds gave a complete course of helminthology, and treated, as fully as was required, of the development, symptomatology, and therapeutics of the parasites of the domestic animals; and in the Dick Veterinary College, I believe I am correct in saying that my own lectures on this subject had precedence of those of Cobbold in London. Notwithstanding this, I have no hesitation in saying that there is no subject connected with the veterinary art which opens up a wider, more profitable, and more beneficial field of action than does veterinary parasitology, and that it is the duty of all veterinary associations, having surplus funds at command, to assist those who have time and inclination for the work to carry out investigations, having for their object the elucidation of its many mysteries.

In connection with the subject of parasitology, I have pleasure in bringing before your notice two specimens of parasites—one a tapeworm, the other a threadworm—removed by Mr. Mulvey, of Bishop Auckland, from the cæca of a salmon trout.

These worms have been identified, provisionally, by Dr. W. Evans Hoyle as the *Bothriocephalus proboscideus* and the *Ascarus obtusocaudatus* vel. *A. acus* respectively. They are of interest mainly as showing the universal distribution of these creatures. I have myself removed a similar nematoid from the muscular tissue of a haddock.

Mr. W. O. WILLIAMS then exhibited a fine specimen of hermaphrodite from a two-year-old stirk (?), there being two bladders, a uterus, and penis, presented to the New Veterinary College Museum by Mr. Arch. Robinson; and also exhibited a very large number of perfectly round calculi from the bladder of a two-year-old stirk, and presented by Mr. Anderson Alford to the museum.

Some discussion followed, after which a vote of thanks to the Chairman terminated the meeting. W. OWEN WILLIAMS, *Hon. Sec.*

LINCOLNSHIRE VETERINARY MEDICAL SOCIETY.

A MEETING of the above Society was held at Grimsby on July 8th. Present: Messrs. T. Greaves, R. T. Hardy, T. Smith, Field, Osborne, F. Spencer (Treasurer), C. Hartley (Hon. Sec.), and C. M. Seely, a visitor.

J. Gresswell, Esq., of Peterboro', having declined the office of President, R. T. Hardy, Esq., the retiring President, was unanimously re-elected.

T. GREAVES, Esq., gave a very interesting paper on the "History and Management of Veterinary Medical Associations," after the ordinary routine of business had been transacted.

The members afterwards dined together at the Royal Hotel.

ROYAL COUNTIES VETERINARY MEDICAL ASSOCIATION.

(*Continued from page* 286.)

An interesting disscussion took place.

Mr. WALKER said they owed a debt of gratitude to Mr. Raymond for bringing this forward ; thus enabling older members to revive their memories and create a new appetite for such subjects. In answer to the first question, he said that as far as he had been able to judge, his opinion was opposed to spontaneous generation. If they once admitted the theory of spontaneous generation, they would have no end of new diseases ; they would crop up every day. He did not think that they had any new diseases, but much more light had been thrown upon old ones, and they were able to define them better. If the theory of spontaneous generation was admitted, it was opposed to the microbe theory, because, if certain diseases were dependent upon certain germs or microbes, then these germs would only produce certain diseases, and that they could only be produced by certain microbes or germs. So diseases did not arise spontaneously. The same argument went some way to answer the second question. He did not think, however much they might cultivate a germ, that they could produce anything except that germ, or that it was possible by cultivation to produce another disease except that originally intended. He did not think that non-pathogenic germs could become pathogenic, however much they cultivated them. For instance, nothing could be produced from a rose but a rose.

Mr. FLANAGAN quite agreed that they were greatly indebted to Mr. Raymond for presenting this important subject before them in such an able manner. He believed the presence of bacteria had existed from time immemorial in certain forms of disease, but had only recently been discovered. He read an article from the *Times* of July 5th relating to the communication of Scarlet Fever by milk. It was a very fatal disease, and if they could discover a cure for it, they would confer a great boon upon society. With regard to Swine Fever, there was a great dissimilarity in cases. He described cases where symptoms of ulceration of the bowels were shown.

Mr. H. G. LEPPER said the symptoms of salt-poisoning were very analogous to Swine Fever. Animals were off their feed, and a *post-mortem* showed no ulceration. He invariably found ulceration of the bowels in cases of Swine Fever. Bacteriology was a science recently come under their notice, and at present it was somewhat clouded in obscurity. He hoped they would all prosecute this study with pains and care, and they would then be able to throw more light upon the subject than at present.

Mr. DREWE said the changes that took place in diseases of the body, according to the disposition of the subject, pointed out that it was possible for a non-pathogenic germ to become pathogenic ; that a harmless germ, under certain conditions, may become capable of harm. There was no doubt that there were special germs connected with certain specific diseases. With regard to Swine Fever, they must take into consideration the peculiar condition and breed of the animals. According to the virulence of the disease, they got morbid symptoms. If the animal died after having had the disease a long time, there they had ulceration. Swine Fever was very difficult of diagnosis, but a great deal depended upon the virulence and duration of the disease.

Mr. BARFORD said that, as a practitioner of forty years' standing, this subject was comparatively new to him, for such was not brought forward when he was at college. Older men, therefore, had more reason to thank Mr. Raymond for bringing this subject before them, and they had heard enough to impress them with the importance of this study. It was very desirable

that they should invite the medical profession to co-operate with them in this matter. Nothing could throw more light than this study of this subject upon the spontaneous outbreaks, such as Cattle Disease, Pleuro-pneumonia, etc. It seemed to shock their feelings when they saw most extraordinary outbreaks taking place, which the strictest investigation could not lead to the cause. But upon entering upon this subject they were shown with much clearness that these diseases were not of spontaneous origin. When they found that these germs of different kinds were disseminated by the wind from one place to another, it threw a new light upon these matters, and it was their duty to investigate the subject to the best of their power. It would assist them to deal better with those diseases communicable from animals to man. He thought very often they formed opinions too hastily on Swine Fever. The symptoms were very vague indeed, and great discretion was necessary in forming an opinion thereon.

Dr. MAJOR begged to be allowed to thank the members for permitting him to listen to this interesting discussion, because, as a country practitioner of twenty-five years' standing, he had little opportunity of following out the study of pathology and practical physiology. He thought there was a very intimate connection between the medical and veterinary professions, and remarked that he had followed with interest Pasteur's process of inoculation, but confessed he was a little sceptical as to its success. He thought there was some laxity in taking the history and details, which should be done in cases of that kind. Notice should be taken of the peculiar constitutions of people, and he was afraid they had much to learn in this matter.

Mr. VERNEY said that, as an inspector under the Contagious Diseases (Animals) Act, he had many cases of Swine Fever come under his notice recently, and all of them had shown symptoms of ulceration.

The PRESIDENT, having alluded in high terms to Mr. Raymond's paper, said he thought it best to have a scientific subject for discussion, so that the younger members would be reminded of the course of studies which they had recently left. Life was too short to allow of a veterinary surgeon working up his practice and of following up his studies ; they ought to make their competency and then devote themselves to the prosecution of this study. They had heard it stated that a German professor had discovered six different germs in the nasal discharges of a horse suffering from Influenza. They might take each one of these microbes and readily believe that it was the cause of the Influenza, but, in order to prove their case, one of those micro-organisms must be found in the dead carcase of the subject ; it must then be cultivated in the proper media and produce the same microbe that was obtained from the subject. It must then be introduced into a healthy subject, and produce the same disease in that healthy animal ; and, on the death of that animal, the same microbe must be found in that subject. The theory of spontaneous generation was a very convenient one to nurse, but it was a very dangerous theory. In the case of Glanders there was sometimes but little to show the practitioner what the animal was suffering from. In time characteristic symptoms developed. He only brought this forward as an instance, and he might also mention Hydrophobia, which was a specific disease impossible to be produced in any other way than by the poison of a rabid dog. He could not accept the theory of Pasteur until members of their profession had had an opportunity of investigating and reporting on the method. Dr. Fleming stated that he believed that some sixty per cent. of persons bitten became affected with Hydrophobia. He did not think so, and argued that ninety-nine out of every hundred medical gentlemen had never seen a case of Hydrophobia. It was not so prevalent in England because of the protection given by clothes. As to Swine Fever, he remarked upon the difficulty attending the diagnosis, and said he must be a bold man who could say he

had never made a mistake. In his experience of symptoms of Swine Fever he found that where there was unusual redness of the skin, the search was in vain for ulceration. Where there was extensive ulceration of the bowels there was not much skin affection.

Mr. WILSON, speaking on the subject of the paper, described an outbreak of Anthrax in a most remote part of his district, and it was impossible to account for it. The animals were buried in a field, and some few years after there was a fresh outbreak. It was then found that the animals had been feeding on turnips grown in this field, and the spores were in the dirt around the roots. He gave other instances of the remarkable outbreaks of Anthrax in remote parts where one farm suffered and other adjoining farms were left free. If they did not get spontaneous outbreaks how were they to account for these mysterious outbreaks?

Mr. WHEATLEY said that he had recently found a decided ulceration of the skin, dermis and epidermis, in cases of Swine Fever, and, when he had made a *post-mortem*, he found no ulceration of the bowels.

Mr. GREAVES expressed his opinion that there was no such thing as spontaneous generation. He mentioned a case where a stable in which a horse had Glanders was locked up for twenty years. After that period a horse was put in there, and he immediately had the Glanders. Could they say how these germs lived through those twenty years without food? Did they live in the walls, in the air, or in the ground? It was a study worthy of their best men, and he hoped Mr. Raymond would be successful in his labours.

Mr. RAYMOND replied to the points raised, and a profitable meeting concluded with cordial vote of thanks to the reader of the paper and to the President for his conduct in the chair.

H. KIDD, *Hon. Secretary.*

CENTRAL VETERINARY MEDICAL SOCIETY.

THE sixteenth annual meeting of the above Society was held on the 7th October. The following Fellows attended:—Messrs. F. W. Wragg (President), J. Woodger, Clement Lowe, F. Jarvis, G. A. Banham, J. Rowe, A. Broad, J. Broad, T. Moore, F. G. Samson, F. Oatway, J. Hall Brown, W. Raymond, H. D. Gibbings, W. Roots, S. Harrison, S. Villar, and J. E. Rickards.

The minutes of the preceding meeting were read, and the Council's report adopted.

The resignation of the post of Treasurer, which Mr. Hancock tendered by letter to the President, was then accepted.

Scrutineers being appointed, the election of officers was proceeded with.

Mr. WRAGG proposed that Mr. J. Rowe should be elected President for the ensuing session. Mr. Rowe, he said, joined the Society at its foundation in 1870, had been one of its most active supporters, and had ably undertaken the duties of secretary for eight years.

Mr. Rowe was unanimously elected, and the four Vice-Presidents chosen were—Messrs. J. Woodger, F. W. Wragg, T. Moore, and F. G. Samson.

Mr. J. WOODGER consented to occupy the position of Treasurer, and Mr. Broad was re-elected Secretary.

The following is the report presented by the Council:—

Report of Council for the Session commencing October 1st, 1885.

At a meeting of Council, held on April 12th, it was decided that the trustees should withdraw from the Bank of England the money at present invested in the names of the original trustees, Messrs. Woodger, Gowing, and Rowe,

and that Mr. J. Woodger should have the same in his keeping till it should be arranged to re-invest it.

It was also agreed that the account should be paid for the stained-glass window put up in the rear wall of the Board-room at Red Lion Square, and money was voted for a stained-glass window to be fixed in the centre of the front wall of the Board-room, the Council of the Royal College of Veterinary Surgeons having accepted the proposal and notified their thanks to the Society.

At the Council meeting held on June 3rd, Mr. Burrell's resignation of his trusteeship was accepted, and Mr. Wragg was elected in his place, the other trustees being Messrs. Woodger and Rowe.

A Council meeting was held on the 23rd September, and the date of the annual dinner was fixed for the first Thursday in November. Messrs. Wragg, Rowe, Moore, and the Secretary were appointed as a committee to make the necessary arrangements. Designs for the proposed window were examined, and that sent in by Messrs. A. L. Moore and Co., of Southampton Row, was accepted, the committee being urged to have the work commenced without delay.

The following gentlemen have, within this session, been elected Fellows of the Society:—Messrs. H. D. Gibbings, W. Willis, J. E. Rickards, W. M. Tegg, and W. F. Raymond.

Nine general meetings have been held during the session, the average attendance of Fellows being thirteen.

The annual meeting for election was held in October, and in November the annual dinner, preceded by an inaugural address by the re-elected President, Mr. Wragg.

At the December meeting, Mr. F. Smith read an essay on "Air in its Relation to Health." Specimens of calculi were exhibited by Mr. Hall Brown.

In February, Professor Tuson delivered a lecture on "Disinfectants." At the meeting in March, Professor Tuson concluded his lecture, giving experimental illustrations. Mr. J. R. Cox showed an interesting pathological specimen, and related the history of the case.

On May 6th, Mr. J. Steel's essay on "Relapsing Fever"—previously printed by the Society—was discussed, Dr. Griffith Evans attending to describe his experience of the disease.

At the June meeting, Mr. S. Villar read an essay on "The Common Parasites of the Domestic Animals"; and in July, in accordance with a resolution of the Council (there being no essay promised), the subject of Purpura Hæmorrhagica was selected and discussed. Mr. Roots introduced the topic with a short paper.

ALFRED BROAD, *Hon. Sec.*

LANCASHIRE VETERINARY MEDICAL ASSOCIATION.

THE usual quarterly meeting of the above Association was held at the Blackfriars Hotel, Manchester, September 8th, 1886.

There were present Messrs. P. Taylor, Thos. Greaves, Wm. Whittle, W. A. Taylor, A. Leather, T. Hopkin, A. Lawson, T. Briggs, J. B. Wolstenholme, C. E. Challinor, W. Woods, jun., S. Ellison, J. M. Bennett, A. New, and J. W. Ingram.

Mr. Peter Taylor was voted to the chair.

The minutes of the last meeting were read and confirmed.

Mr. Hopkin then read a letter he had received from Mr. H. Ferguson, with respect to the Presidency; he begged to be excused taking office, and thought that it would be much better if Mr. Briggs would reoccupy the chair.

Mr. HOPKIN proposed, seconded by Mr. W. A. TAYLOR, that Mr. Briggs be re-elected President.—Carried unanimously.

Mr. BRIGGS thanked the society for re-electing him. He was pleased to continue in office, seeing that the debate on the ninth clause was now in abeyance, owing to the altered condition of the Council.

The resignations of Mr. E. Faulkner and Mr. J. S. Hurndall were brought forward, and it was resolved that the Secretary do write each of these gentlemen, asking them to withdraw the same.

Mr. GREAVES said that, as it was the wish of the Association, he had great pleasure in withdrawing his resignation.

Mr. W. WOODS, Jun., then read the following paper on

THE PROBABILITIES OF EVOLUTION IN DISEASE-PRODUCING GERMS.

Mr. PRESIDENT AND GENTLEMEN,—May I commence this paper by saying, in explanation of one so young as I appearing before you as an essayist, that my object in writing this paper is to learn and not to teach. I have often been asked to give an essay on any subject I chose, but have always refused, for reasons I need not now explain, but when, at last, I came to the conclusion that I ought to do something, I, at the same time, came to the conclusion that it would be impertinent on my part to read, what is called a practical paper, to a lot of gentlemen who had been in practice two or three times as long as I. Having thus nothing but a theoretical paper lef for me, I determined to take up a subject that I have thought considerably about, but, as I have just said, I bring it before you more that I may learn, than that I can hope to teach, as I know full well that young people when they commence to theorise are apt, from want of experience, to become so extravagant, that the result of their reasoning can easily be reduced to the absurd by more experienced minds.

Although I have christened the paper one on evolution in germs, the title is not strictly accurate, because instead of holding that one species may gradually develop into another species, I am strongly of opinion that this is impossible. Perhaps I had better say a few words upon the meaning of the word.

The present significance of the term "evolution" is widely different from what it was originally. Its meaning was at first synonymous with development, for it was the term used to mean the expansion of that which was invisible into visibility. Thus, it was considered that the ovum contained all the different parts of the adult in miniature, not exactly that a man was a magnified ovum, but very nearly. This soon was combatted by the theory that, although the germ did not contain the formed adult organs in miniature, it was an original preformation capable of producing them. Thus, that which originally meant an unfolding or expansion, became used in a wider and wider sense, until it embraced the idea that the physical world, and all things in it, whether living or not living, originated by a process of change due to the continuous operation of purely physical causes, out of a primitive, relatively formless matter; consequently it includes all theories respecting the origin and order of the world, which regard the higher and more complex forms of existence as following, and depending on, the lower and simpler forms. This last, and most extended meaning, I am heartily opposed to, my object merely being to draw your attention to what I consider leads one to the belief, that a development in the varieties of germs does not occur, and that at various stages of that development you attain to various classes which, when introduced into the system, produce results so very different, though arising from the same germ, that we class those germs as different because we recognise the results they produce as different diseases. It does not seem very difficult to prove this. The small practical result which may obtain from this proof, I shall refer to at the end of the paper, saying nothing now beyond that, if it is proved, it will at any rate explain why Cholera in the human being and Cattle Plague in the lower animals should so often be concomitants—why, when Cholera is dying out in a district, it often partakes

more of the character of Typhus Fever, and it would point to the conclusion that, in order to stamp out one contagious disease, you should at the same time endeavour to stamp out any prevalent disease to which it is closely allied.

I shall commence by considering shortly what are the present opinions with regard to germs, and then proceed to consider what are the arguments in favour of the opinions I hold. There are, then, pathologists holding, with regard to the origin of germs, two very opposite opinions. By far the larger class hold that the germs of disease always arise from pre-existing germs, and the smaller class hold the opinion that contagious diseases can originate *de novo*. I need scarcely say, that I am a firm believer in the former of these theories, although my reasons for that belief differ a little from some other people's. For example, in order to explain why a disease occasionally breaks out without any similar pre-existing disease, it is assumed that germs are almost indestructible by natural means, that they can live for an enormous length of time, and the moment they find a suitable habitat, consisting, perhaps, of a combination of conditions, that they can immediately commence to thrive, and in proof of this, it is shown how mummy wheat a couple of thousand years old will grow if planted in the ground. There seems to me to be one very weak spot in this theory, and that is, that the conditions are remarkably different, the diseased germ being continually exposed to the atmosphere, whilst in mummy wheat the germ has, so to speak, been hermetically sealed up and kept perfectly dry for all these years, and not exposed to atmospheric influences at all, and I hold that it is against all reason to suppose that these small germs alone, amongst created particles, can withstand the disintegrating influences with which nature surrounds us. I consider that germs, like everything else exposed to atmospheric influences, will decay and die, and my explanation of contagious diseases arising apparently *de novo* is this, that in all likelihood there are other media besides the fluids of the body in which germs can thrive and multiply, but that those other media may not always be equally acceptable to the germ, and hence you get, by a natural cultivation, an attenuated germ incapable of producing the disease, which germ may go on reproducing itself time after time in successive generations, not the same germ note living for ever, but successive crops, until, by some circumstances, unknown perhaps to us, there is from this attenuated germ what I would call a backward cultivation, or reversion, capable of producing disease, and thereupon the disease will come about apparently *de novo*. This seems to me to be a more likely explanation than one and the same germ can live for ever. I have no sentiments in common with the spontaneous generationists, for the simple reason that I cannot comprehend the possibility of any fortuitous concourse of atoms uniting to form a living germ. The first disease I shall refer to is that peculiar one known as Ague, or Roman Fever, which is seen only in marshy districts, and which is now almost extinct in this country. My reasoning lies as follows:—Owing to drainage, the disease has practically disappeared in Lincolnshire and the Fen country generally, but it must be allowed that, provided the district were permitted to again become a swampy marsh, Malarial Fever due to the same organism would certainly reappear. But, if so, where is the germ to spring from, and what has it been doing during the interval? Well, it seems to me that what I have just stated is a far more probable method of explanation, than to consider that the germs of every possible disease are always everywhere awaiting a favourable opportunity of multiplying, for I take it that it is not the presence of the marsh which produces the disease, but the presence of the germ, and that, if the germs were everywhere, we should have the fever everywhere, unless we have to take it for granted that the germs are compelled to go through an intermediate process in vegetable or animal matter, like the liver fluke of a sheep before they are in a condition to produce their recognised effects in the body. We all know that Pasteur and others have taken germs, and, by cultivation,

have so attenuated them that they become innocuous; and I think that it is not unreasonable to suppose that what Pasteur has done artificially might possibly be done naturally. In other words, that, by reclaiming waste marshy lands, you so attenuate the germ of Ague as to render it inert, and that as Pasteur has, by an opposite cultivation, caused these organisms to regain all their virulence, that in the same manner, if these reclaimed lands were allowed to return to swamps, that there you had, by natural means, a similar opposite cultivation, whereby these attenuated Ague germs would regain their Ague-producing power. I think the fact that Roman Fever may actually become a a contagious disease, if the fever patient is surrounded by bad sanitary conditions, points strongly to the same conclusion. But then the question arises, Do you, by attenuation, produce another germ, or do you merely draw its fang? This is a difficult question. We know that one observer, and one alone, has stated that he has succeeded in absolutely changing the innocent bacillus subtilis of hay infusions into the most malignant bacillus anthracis of Splenic Fever. Now this statement stands alone, and must, I think, unless corroborated, be received *cum grano salis*, but let us consider what are the arguments in favour of its being true. Pasteur, as I said, has so attenuated the germs of Anthrax as to make them not only innocuous, but absolutely as to secure immunity from the deadly action of the Anthrax-germ when implanted in the body after inoculation with the attenuated germ. That is to say, he has actually produced a germ which is antagonistic to the germ from which it sprang. This may either be looked upon that he has produced a disease which is not Anthrax, but which is antagonistic to Anthrax; or it may be looked upon, that he has produced a germ which, when inoculated into the system, either destroys the particular pabulum which the Anthrax-germ requires for its development, or has so altered it as to make it an unfit habitat. Now there seems to be here a strange analogy between Pasteur's attenuated Anthrax-germ and its effects upon the subsequent inoculation of the true Anthrax-germ and the action produced by the inoculation of the virus of the various variolæ of different animals. It would seem from analogy as if the Cow-pox virus were the attenuated virus of Small-pox. How attenuated I cannot say, but a consideration of the report of the French Commission appointed to consider the question of the various variolæ will show the analogy closer. This Commission was appointed a few years ago when the question of the identity of, or connection between, human Small-pox and Cow-pox was in dispute, and their conclusions presented in a report, after careful experiment, were to the effect that these two diseases, *i.e.*, Small-pox and Cow-pox, were not the same, although so very similar in character, that if you inoculated a cow with the virus of Small-pox you produced a papule or pimple with a small area of inflammatory tissue around, but that you could not produce the several stages of Cow-pox. They proved further that the two diseases were positively different, insomuch as you could have both Small-pox and Cow-pox going on in the same animal at the same time. Following this, they proved by experiment that the two diseases, though not identical, were antagonistic, *i.e.*, that if you inoculated with either Small-pox or Cow-pox, and allowed it to run its course, it gave immunity from the other disease. But the Commission went further than this. They proved that Cow-pox was not Sheep-pox, by which I mean that inoculation with Cow-pox virus in a sheep did not produce Sheep-pox, but that, as before, it gave immunity. Now by analogy this seems to prove that Pasteur in attenuating the Anthrax-germ produces a new germ, which produces a new disease, not Anthrax; but a disease antagonistic to it, as is the case with the Cow-pox and Sheep-pox germs, and this, again, would point to the conclusion that in the variolæ, by some process similar to attenuation, you have the same original germ altered somewhat in its character by successive cultivations, and which is responsible for the whole of the variolæ of different animals. This seems to me to be

more feasible than to suppose that for diseases having all their characters in common, and yet having been proved to have been not the same diseases, that you have for each a totally separate creation, having no connection the one with the other, for ever floating in the atmosphere, ready to pounce upon the particular animal it is especially made for. Even the germs of Anthrax itself are now being differentiated, Splenic Fever and Black-quarter having each their own particular germ, differing from each other merely in size. Yet who, having seen both diseases, notwithstanding this latest discovery, can doubt but that there is a close connection between them—a connection not explained by the assumption of two distinct creations. I may mention, again, that the known fact that Roman Fever may or may not be contagious, according to the sanitary conditions of the patient suffering from it, points strongly to the conclusion that germs may, by conditions favouring their development, produce different results; and I contend that the difference between the same germ being either contagious or not, according to circumstances, is equally as great as the difference between Splenic Fever and Quarter-ill.

To take another disease. In Pleuro-pneumonia Contagiosa it is proved that the virus of this disease can act differently if merely implanted in another part of the body. What happens in inoculation for Pleuro-pneumonia? Do you produce Pleuro-pneumonia? Well, Mr. Rutherford, of Edinburgh, who has inoculated some thousands, and who has studied the subject closely, practically says, in the same breath, both that you do and that you do not. He says, You do produce true Pleuro-pneumonia, but, he says, the Pleuro-pneumonia which you produce is not infectious. Now Pleuro-pneumonia is an infectious disease, and I say, for this reason, he does not produce true Pleuro-pneumonia. His reasoning, however, is very good. He says Pleuro-pneumonia is a fever, the result of which is an exudation into the tissue, which was the seat of the reception of the pleuro poison, and that, as it is a poison only disseminated atmospherically, that is the reason it is always in the lungs; that he produces the same pathological conditions at the end of the tail, but that it is not infectious, because a cow does not breathe from the tail. At least, this is as I understand it. It seems to me a remarkably clever theory, but there are some very awkward questions to answer with regard to it. Why is it, for example, that you frequently see almost the whole of one lung invaded with Pleuro and completely consolidated, whilst the other may be quite free? and why is it that the right lung should oftenest become affected? And then, again, I never heard of Pleuro of the stomach, although, from the cough and expectoration, it must be supposed that at some time many animals must have ingested some of the infected material, and that if the Pleuro poison affected only the seat of reception, Pleuro of the stomach must undoubtedly occur. And, moreover, in inoculations at the tail, the results which occur if the tail be not amputated are necrotic in their nature, huge gangrenous masses sloughing away, accompanied by suppuration; while in the lungs, although the atmosphere has equal access to them as it has to the tail, the results in the lungs are, I think I may say, not of a necrotic or suppurative nature. It seems to me that an easier explanation is, that the different results are due to different conditions. In the tail the pabulum is such that the virus reproduces itself, but in so doing is somewhat attenuated, and hence produces different results, and, as proving that such may be the case, I would point to Cancer in the human being, where, whilst the epithelium is external to the true skin or other membrane, it is perfectly innocuous, but where, by a down-growth of epithelium, you get epithelial cells embedded in connective tissue, there those cells assume a very different character, consuming that tissue, and forming that terribly formidable disease, Cancer. I mention this as showing that, even in different parts of the same body, cells may have a different action, and if cells, why not germs?

The only practical leaning of this paper, so far as I can see, is this: if there

be any probability of evolution in germs, it should teach us to be doubly careful in sanitary arrangements, including light as well as ventilation and drainage, for on the principles of evolution one can quite understand that, without these precautions being thoroughly attended to, animals are continually liable, from reversion of attenuated germs, to attacks of contagious disease. If the principle of evolution is not admitted, it makes it far more difficult to explain how it is that Glanders so frequently breaks out on board ship, where animals are herded together, and surrounded by insanitary conditions. Where all these animals have for some time previous been free from Glanders, and where they have not been exposed to the contagium of Glanders, it seems to me insufficient to suppose that some one of these animals must have had Glanders in a latent form. With regard to hygienic arrangements I would only say, that in my opinion no stable or cowshed should be without good light, the window if possible being so situated that the sun shines upon it during some period of the day. The ventilation should consist of openings both near the floor and at the roof. You then do not depend entirely upon the diffusion of gases, but create a current in which the cold air enters at the bottom, whilst the hot carbonic acid, etc., pass out at the roof. The best drainage with which I am acquainted is that as carried out at Newmarket, where, if the Irishism may be allowed, the stables are not drained at all. The floor consists of a solid bed of concrete, which may or may not be channeled, the whole inclining slightly away from the horse's head. This to my mind is the perfection of drainage, because it is not fresh but decomposing organic matter which is hurtful, and by this system the stable-floor must be washed every day to keep it clean. Nothing can penetrate or permeate it, and it is not slippery either wet or dry.

It may seem to some gentlemen that throughout the paper I have been closely following the teachings of Darwin, and naturalists who think as he does. Perhaps I may have been to some extent, but with this decided difference, that while I think the teachings of science are decidedly for, and those of religion are not against the idea, that there may be a certain amount of change of variety in both animals and vegetables, I cannot think that a distinct species can ever by any series of gradations develop into another distinct species.

I mean to say that where in endeavouring to trace varieties you come to a vast chasm—a missing link—that to endeavour there to prove that the one is descended from the other, is to open a vast field for doubt, an endeavour in fact to prove the impossible. I would conclude by repeating that I am not endeavouring to prove this, but only to show that it is possible that many of the disease-producing bacteria may have their varieties changed by natural as well as by artificial cultivation, and that thus one bacterium, which at one time produces changes in the system which we recognise as a certain disease, may by natural cultivation undergo such slight variations that it may induce other changes in the system which we recognise as another disease.

The paper was exceedingly well received, and an interesting discussion followed. A vote of thanks to the essayist concluded the meeting.

JAMES W. INGRAM, *Hon. Sec.*

WESTERN COUNTIES VETERINARY MEDICAL ASSOCIATION.

THE sixth meeting of the above Association was held at the New London Hotel, Exeter, on September 23rd, C. Parsons, Esq., the President, in the chair. There were also present Messrs. J. P. Heath, W. H. Bloye, J. A. Collings, T. Olver, W. Roache, R. E. L. Penhale, W. Penhale, Jun., G. H. Golding, F. Airey, J. H. Penhale, and the Secretary.

Letters and telegrams notifying inability to attend the meeting were received from Messrs. Elder, Thomas, and Ascott.

The minutes of the last meeting were read and confirmed.

The PRESIDENT, on rising to call on Mr. Heath for his paper, remarked that he saw but little chance of Clause IX. of Supplemental Charter being rescinded. Some in high places, who but recently were dead against the clause, had become sudden and remarkable converts in its favour. In country places any one who chose was still allowed to practise the science and art of a veterinary surgeon, very much to the detriment of the younger members of the profession. He hoped that such a state of things would not long continue to exist. He then called on Mr. Heath to read his paper on

COMMON FORMS OF BLOOD-POISONING.

Mr. PRESIDENT AND GENTLEMEN,—It is not my intention to-day to enter into the pathology of those complaints among our domestic animals which we have all studied, and which may be found described in the works of our veterinary authors as "Black Diseases," under the titles of *Murrain*, *Black-quarter*, or Quarter-evil (Hemato Sepsis), *Hæmo-Albuminuria*, *Sanguineous Ascites*, *Splenic Apoplexy*, and other diseases of an allied character, but to mention some common forms of Blood-poisoning which every experienced practitioner must have come across in exercising his vocation. It may be said, after I have finished, "You have told us nothing new!" to which I would reply, "There is nothing new under the sun;" but if these few words draw the attention of *especially* the younger members of the profession, to common states of the animal system (often perhaps overlooked because they are so common), and thereby increase the usefulness of some practitioners to the owners of live stock, they must tend to elevate our profession in the esteem of the public in general, and, therefore, will not have been written in vain.

It will be best, I think, to narrate a case of each kind, with the symptoms and the treatment which has been successfully adopted, as a sample or illustration of the above forms of disease which have come under my personal observation.

The first that I can recall to mind is that of seven yearling bullocks, which I was requested by the owner to come and see, as he had tried all the remedies in his quack medicine chest, and puzzled his own experience, which was a long one, with the result that the animals obstinately persisted in getting worse instead of better. I found the whole of them very stiff in their limbs, some scarcely able to bend their joints, using their legs more like trestles than as if possessed of joints, and one was entirely off its legs, and unable to rise. Some were feeding a little, but several had entirely lost their appetites; their pulses were feeble, temperature high, and they were almost in a prostrate condition. On finding their food to be wholesome, I next asked to see where they drank, and found that the place was within a yard or two of the exit of the main drain of the house into a small stream, which was nearly dry, owing to the drought.

Of course I at once forbade the use of this water, kept them out of the plot of land altogether, whilst I administered gruel and stimulants, as ammonia and spirit to the worst cases, with carbolic acid in small doses internally, and was hereby successful in saving the whole of the animals, although it was several weeks before those most severely attacked entirely shook off the effects of the morbific matter which had been imbibed into their systems evidently through impure water.

The next cases were those of two cows, out of a herd of six or seven, which showed genuine symptoms of Typhoid Fever—loss of appetite, high temperature, a fœtid Diarrhœa, an offensive smell with the skin of the whole of the body, partial depilation, and considerable stiffness of the limbs. This was during a season of drought, and I attributed it, as the field was adjoining the city cemetery, to drinking the water contaminated with the drainage from the

cemetery. As I have had cases of a milder character on the same ground since the above in seasons which have been especially dry, I feel confirmed in this opinion. I treated these animals similarly to the last—gruel, beer, and other stimulants, with small doses of carbolic acid and salicylic acid internally—being successful in saving both animals, with the result, in the worst case, that the whole of the hair of the body came off within a few weeks, and the new coat never afterwards approached the strength or thickness of the old one as long as the animal lived, always being of a soft, silky nature, and very thin.

I was called in for an exactly similar case, as a second opinion, not long since, the drinking-water being mixed with sewage, but the animal was in a dying state, past recovery, and died a day or two afterwards. I made a *post-mortem* examination in this instance, and found every organ in the body impregnated with pus and in a state of disintegration.

Another form of blood-poisoning is from deep-seated wounds, like Poll-evil, from which the pus is unable to obtain free exit; or from internal injuries, where there is destruction of tissue and, consequently, decomposition of certain parts of the body, which become absorbed into the system, enter into the circulation, and thus poison the blood. I have known as many as twenty abscesses in different parts of the body, result from a case of Poll-evil, in which the pus had not been evacuated, resulting in general Pyæmia and death.

Again, *Irregular Strangles* is a common cause of blood-poison where the abscesses have not discharged in the usual manner, either from debility of the animal's constitution, or else where the abscesses have discharged themselves into one or other of the internal cavities of the body, and have so become absorbed into the system; but such cases have been described, or at any rate mentioned, by other authors, under the head of Pyæmia, as one of the results or terminations of the disease.

The most general form of blood-poisoning—of every-day occurrence—is among the female sex of all our domestic animals, either after abortion or parturition. The mare, the cow, the ewe, the bitch, and occasionally the sow and the goat. Few of us who have been any time in practice have not seen an animal after abortion in a state which the farmer calls "doing badly," and yet no *violent* symptoms are at the time apparent. If the history of the case is inquired into, the probability is that the whole of the placenta has not been discharged, or the viscous fluids of the uterus have been left to decompose within the animal, so becoming absorbed into and vitiating or poisoning the blood. The same state of things is of frequent occurrence in the smaller kinds of pet bitches.

But this is a very general state of things in delicate animals after ordinary parturition, more especially ewes and cows. I believe that thousands of ewes are annually lost in this country from this cause alone. Farmers, as a rule, so seldom send for a veterinary surgeon to treat a ewe *after* lambing (whatever they may do in a case of difficult parturition), that few who have not the experience can imagine the frequency of such cases among ewes. I have many times taken off as much as a pint of putrid or semi-putrid fluid from the uterus of an ewe a day or two after lambing. Only this summer, I had a cow under my care which calved a month before time and was in a critical state for a fortnight after, continually discharging bloody and putrid fluids and portions of the placental membranes. She ate nothing for days and had to be supported with gruel, spirits, and vegetable tonics, treated with medicines which produce contraction of the uterus, and the parts syringed daily with warm water and Condy's fluid (permanganate of potash), until the natural contraction of the uterine muscles took place.

(*To be continued.*)

MIDLAND COUNTIES VETERINARY MEDICAL ASSOCIATION.

THE fifty-eighth quarterly meeting of the above Association was held on September 28th, 1886, at the Star and Garter Hotel, Wolverhampton.

Mr. A. Over presided, and there were present, among others, Professor Axe, Mr. J. H. Cartwright, Mr. F. Blakeway (Treasurer), Mr. T. Greaves, Mr. J. H. Reynolds, Mr. Geo. Smith, Mr. H. M. Stanley, Mr. Malcolm, Mr. H. Collett, Mr. Meek, Mr. W. Dale, Mr. R. Verney, Mr. A. Spruce, Mr. H. Pritchard, Mr. C. T. Barnes, Mr. R. Trigger, Mr. E. Beddard, Mr. W. Carless, Mr. Wragge. Mr. Golden, Mr. J. H. Cartwright, Jun., Mr. R. Pritchard; and visitors—Alderman Gibbons, Mr. Butlin, Mr. Whittle, Mr. Pemberton, and Mr. Dawes; with Mr. Thomas Chambers, Hon. Secretary.

Letters of apology were read from Professors W. Williams, McCall, and Walley (Edinburgh), Mr. Meneeh (Northampton), Mr. Taily (Birmingham), Mr. Simpson, Mr. A. Hodgkins (Hanley), Mr. Perrins (Worcester), Mr. Barling (Ross), Mr. Ison (Atherstone), Mr. Bland (Alfreton), Mr. Goodall (Melton Mowbray), Mr. Freer (Uppingham), Mr. Cross (Shrewsbury), Mr. Barling (Hereford), Mr. Drewitt (Gloucester), Mr. Tipper (Birmingham), Mr. Green (Dudley), and Captain Russell (Grantham).

MR. BLAKEWAY rose, previous to the reading of the minutes of the last meeting, and stated that he wished to refer to a loss sustained by the Association since their last meeting. Mr. Sampson Gamgee had passed away from among them. The deceased gentleman, not only stood high in their Association, but he had a high reputation as a literary and scientific man. They had studied and passed together, and it was with pleasure he remembered how many valuable things he had learned from Mr. Gamgee. The profession, as well as the medical faculty, would feel the loss of Mr. Gamgee, and their Association would be remiss if it failed to recognise Mr. Gamgee's great abilities. (Hear.) He begged to propose a resolution, recognising those abilities and condoling with the family of the deceased.

Mr. OLVER seconded the resolution, and said he had been intimately connected with Mr. Gamgee, mainly through the National Veterinary Association. They all remembered his eloquent address in Birmingham, at the Medical Institute, of which he was the President; and they all remembered that he gave them assistance and advice in every way. Therefore, they would be remiss in not passing a vote of condolence with Mrs. Gamgee and family.

The PRESIDENT rejoiced to see the tribute paid so willingly to the memory of Mr. Gamgee.

The resolution was carried unanimously.

The minutes of the last meeting were read and confirmed.

The PRESIDENT, in accordance with notice, proposed that certain instruments be purchased out of the funds of the Association for the use of the members.

Mr. STANLEY was in favour of the proposition, but preferred to know what instruments were to be purchased before giving his vote.

Mr. TRIGGER agreed with the last speaker, and suggested that a sub-committee should deal with the matter.

The PRESIDENT formally moved his resolution.

Mr. TRIGGER seconded it.

Mr. STANLEY said the instruments were few in number, and he failed to see what use they would be thirty or fifty miles off.

The HON. SEC. said the Treasurer had told him there was plenty of money in hand, and the proposed purchase of instruments of an expensive kind would be useful to their poorer brethren. They would probably want Thompson's tooth-shears, Professor Axe's lithotomy instruments, ecraseur, etc., and those could easily be sent forward on receipt of a telegram.

Mr. GREAVES said other institutions had them, and it was the right thing

for the Association. In the Border Counties and in Yorkshire they had instruments to lend. £10 or £15 might very well be spent by a committee to whom the question could be left.

Mr. MEEK suggested that the President and Hon. Secretary should have power to spend £20 in the purchase of instruments seldom used.

Mr. CARTWRIGHT seconded, and the resolution was carried unanimously.

Mr. MALCOLM moved, and Mr. PRITCHARD seconded a resolution that Mr. Stanley should assist in the purchasing of the instruments.—Carried.

The PRESIDENT then called upon Professor Axe to deliver a lecture upon his experiences in Lithotomy and Lithotrity. He had to thank the Professor in anticipation for the lecture, inasmuch as there had been a difficulty in securing a paper for that meeting. Upon being appealed to at a late period, Professor Axe at once responded, and so the best thanks of the association were due to him. (Hear, hear.)

Professor AXE said he never considered it too late to render service to the profession to which they all belonged. (Hear, hear.) It had been usual in reading papers or delivering addresses on such occasions to deal with pathological views of diseases, and the surgical views had been neglected. He had, therefore, chosen the subjects of Lithotomy and Lithotrity as interesting ones, and he proposed to give them his recent experiences. He also proposed to explain to them the shape and construction of several instruments he had made to expedite the necessary operations in such diseases. He regretted he could not show them the instruments themselves, but the railway company had not delivered them up to that time.

The Professor then delivered his lecture.

Mr. WRAGGE said he had listened to the lecture with a great deal of pleasure, for its entrance into such minute details made it very valuable. From those details, and the graphic manner in which the operations had been described, he felt that he had gained some valuable knowledge that day. He once had a case in which the sebulous matter had formed a calculus, and when he operated he received a good-sized stone. Upon examining the bladder again, there was found a substance which was thought to be another calculus. He grasped it with the forceps, and found it was a substance attached to the bladder. The patient died from rupture of the bladder, and after the *post-mortem* it was a mystery how so large a clot of matter had been supported by so thin a coat. In conclusion, he had to thank Professor Axe for his lecture.

Mr. TRIGGER agreed as to the indebtedness of the Association to Professor Axe, and thanked him for his scientific treatment in so important a subject. The veterinary surgeons did not have many opportunities of treating such cases, for generally the owners, on being informed of the disease, got rid of the animal.

Mr. BLAKEWAY, Mr. CARTWRIGHT, jun., and Mr. GREAVES mentioned cases they had seen and operated upon successfully.

Professor AXE, in reply to the remarks and questions put, said that particles remaining behind might form the basis of another calculus, but it was a matter one could not avoid—to do great good one had to risk a little. Veterinary surgeons, like other people, could only do their best, and had to be guided by the means at their disposal. In all such cases there were difficulties which it might be impossible to overcome, and there were times when the calculus particles would remain. When there was a thickening of the bladder, the small remains got in between the folds, and all that could be done was to use the greatest care and attention. In the seven cases he had recently had only one had proved fatal, and, without taking any credit to himself, he must say that he believed the instruments were as much to blame as any one, for they were of a clumsy nature, and they had directed his mind to something better. In the case mentioned by Mr. Greaves, of a bladder filled with soft, pasty

matter, it should be scooped up at once. There was no danger in such cases if the bladder was well washed out. The only thing to apprehend was where the bladder had contained such a weight of the matter for so long a time that the walls became paralysed and could not recover. In that case there would be a likelihood of retaining the urine.

Mr. MEEK said the remarks of all the speakers showed how seldom the cases were met with. He himself had only diagnosed two cases in all his time. One of the horses was his own, and he lost it. (Laughter.) The other was that of a client, and he was successful. He, like the other speakers, was obliged to Professor Axe for his lecture.

Mr. CARTWRIGHT, sen., proposed a vote of thanks to Professor Axe for coming among them and giving them such an interesting lecture.

Mr. OLVER seconded the vote of thanks, and said the discussion showed the necessity of having the best instruments, and that reminded them of the resolution passed earlier in the meeting. (Hear.)

The resolution was then put and carried unanimously.

The PRESIDENT moved that Professor Axe be elected an honorary associate of the society. (Hear, hear.)

Mr. TRIGGER was under the impression that this had been done before at Rugby, after Professor Axe had read a paper on the Rot in sheep.

Professor AXE returned thanks for the vote, and for the kindness of the members in listening so patiently to his lecture. He also cordially accepted the high honour bestowed upon him in making him an honorary associate. (Applause.)

It was decided to hold the next meeting at Stoke.

Mr. STANLEY moved, and Mr. MALCOLM seconded, a vote of thanks to the President for his conduct in the chair.

It was carried unanimously. The members afterwards dined together.

THOMAS CHAMBERS, *Hon. Sec.*

THE ALDERSHOT ARMY VETERINARY ASSOCIATION.

THIS old Association has lately been revived. It has for its objects the free discussion of all subjects of professional interest in the Army, and much benefit is likely to accrue from the free interchange of ideas, which these Associations are so eminently calculated to produce.

The first meeting was held in the Army Veterinary School on the 31st August, 1886, when the important subject of the picketing of military horses during peace and war was freely discussed. The experience of several members who had served in India, Africa, Egypt, and Australia was given as to the mode of picketing best suited for those countries.

The second meeting was held on the 13th of October, when the subject of OBSCURE LAMENESS was introduced by Mr. Smith. Specimens were exhibited illustrating some of the forms of Obscure Lameness described.

The further discussion of this important subject was deferred to the next monthly meeting.

We hope to give a brief *resumé* in our next.

PROCEEDINGS OF THE THIRD GENERAL MEETING OF THE NATIONAL VETERINARY ASSOCIATION.

(*Continued from page* 304.)

Mr. HUNTING: After the able way in which Professor Axe has dissected this paper I shall not attempt a scientific refutation, but shall try to take a common-sense view of the question. On reading the paper, I looked first of

all for some clinical value, and was unable to find any; then I looked upon it as a sort of exercise in inductive reasoning, and I found it a logical failure. There are no facts in the paper. There are one or two propositions, from which the essayist has attempted to deduce other propositions, and he has fallen into schoolboy errors of logic. The whole paper is a series of similes and comparisons. It so happened that the other day I came across Professor Bevon's "Logic," where he says:—"Incorrect reasoning consists in putting one thing for another when there is not the slightest likeness between them." This is just what our essayist has succeeded in doing. He apparently draws an analogy between seeds and germs. Now if the seed is the analogue of the germ, the soil must be the analogue of the animal body, and yet throughout the whole paper he stumbles into the strange fallacy of looking upon the air as the analogue of the soil. He tells us what climate or moisture has to do with the development of seeds and germs. The animal body does not vary like that—the analogy does not run. Now let me glance through the paper as the essayist himself has written it, divided into paragraphs. In the first part he says, with regard to the germs of a specific disease, "if the circumstances are unsuitable to its development they will lie dormant. Professor Axe has shown us that though they may lie dormant for a time, there are forces in nature tending to their destruction. The simile about the wheat that was found in a mummy in Egypt would only have a proper analogy by imagining the germ in a hermetically sealed test tube. If they are knocking about in the atmosphere I imagine that they cannot long escape these eternally disintegrating forces. You would not find those seeds alive in the middle of the Atlantic, simply because there is no earth for them to grow on, nor would you expect to find germs which require the interior of the animal body for their reproduction, everywhere floating about in the air. That opens up a very wide question, which I shall not go into at all, *i.e.*, the existence of disease germs outside the body. It is probable there are conditions outside the animal body in which those germs may exist and even develop. As to that, we know very little. We are told English seeds die out in the tropics, and foreign seeds die out here. Outside of their proper position they die—the seeds are killed. I believe it is a fact that Yellow Fever germs are killed by a low temperature, and Cholera is killed in this country because it does not get into favourable circumstances outside the body. He says, "Some people talk of stamping out disease." There is a tone—a sort of implied sneer—in that I do not like at all: I will reply by saying that they not only talk about it, but they do it; they have already stamped out in this country the germs of Cattle Plague and of Small Pox in sheep. They did it by destroying the germs where alone they could exist—in the animal body. The author does not seem to see the force of his simile about the weeds. He says, "Noxious weeds, though indigenous to the soil and suitable to this climate, will not prevail where the husbandry is good." I suppose that good husbandry means making the soil most fit to develop the seeds that are in it. How then account for the weeds perishing? The answer is plain, but it proves exactly the opposite of what the paper wishes to infer. The good husbandman kills the seeds of the weed, just as the good sanitarian kills the seeds of disease, and neither devotes any attention to the air, in which it is suggested all the mischief is floating. All through the paper there are these similes which do not apply and do not run. He says that the germs of disease require "a suitable nidus." A suitable nidus, I take it, is a live animal. "The germs of disease thrive in a warm atmosphere." So they do in cold: Cattle Plague, Glanders, etc., in winter are not decreased. In paragraph 8 he asks, "Where do the germs of disease come from?" and he replies, "They are present everywhere and are spread through drains and sewers." They may be to man, but I am not aware that any diseases spread to horses through drains

and sewers. Such things are media only because they are the most favourable conditions for the spread and collection of disease germs from men and to men. Then we have another miserable illustration about the whity-brown paper. He does not seem to see that the analogy he is attempting to force would only apply to the collection of germs, not their development, and disease means development and growth of germs. There is not a single fact given in the whole paper. Now on the spread of specific diseases we have most reliable and numerous facts. With regard to the diseases of Glanders and Farcy alone, you will find if you once get them into a stable they will spread from horse to horse and you find that it does not affect the weaker horses more than the strong ones. I unhesitatingly say, nothing in the constitution of the horse will prevent it being affected with Glanders if the virus once gets into his system with the food or water. In ninety-five cases out of a hundred one can directly trace the contagion. I leave it to you whether that is true or not. As a matter of logic, if ninety-five per cent. of anything happen from a certain definite cause, we may conclusively infer that the other five per cent. happened from the same cause, although you were unable to trace it. Similar proof would be accepted in the common law-courts of this country as evidence sufficient to hang a man, let alone to gibbet a bacterium. The whole force of the paper seems to me to depend upon a syllogism which may be put thus: "Germs are like seeds; seeds are everywhere; and therefore germs are everywhere; and, to go a little further, germs of disease are everywhere. They only develop in heat and filth; if there be no heat and filth there will be no disease." The fallacy is so transparent I do not think it is worth arguing further about. A fair verdict on the paper is that it contains no facts, that its reasoning is fallacious, consequently that the hypothesis of a wide, general distribution of disease germs is unsupported by evidence or argument.

Professor WILLIAMS: There is one question, namely, as to whether these bacilli or bacteria under certain conditions do not become pathogenic; and I think a reference to this fact will clear to some extent the mystery which surrounds the spontaneous outbreak of some of these specific diseases. It is well known that the Roman Fever is due to a bacillus. In the blood of these people bacilli are found upon microscopic examination. Dogs have been inoculated with the blood of these people, and these bacilli have been developed in the blood of the dogs and have induced all the symptoms of the Roman Fever, as seen on the Roman plain. Undoubtedly the bacilli of Malarial Fever emanate from the ground; they do not induce disease, except upon the Roman plain. Undoubtedly, the source and origin of these bacilli is the ground of the Roman plain. These can enter into the blood of these people and produce Ague, and this Ague under good sanitary conditions, if the person be removed from the Roman plain, is not a contagious disease in the strict sense of the word; provided it be taken into a house free from the influence of marsh malaria, the disease dies out. But, under certain conditions, mark you—and I think it is very important—if these people be taken into unsanitary conditions, such as badly ventilated rooms and what not, then the disease becomes infectious and contagious, and passes from the people removed from the plain of Rome to the people about them. Here we have two different conditions. First the bacillus, after one cultivation in the human body, seems to lose its virulence, but under certain conditions when the surroundings are bad it seems to increase in virulence, and becomes a source for the propagation of the disease to other human beings who have not been subject to the emanations from the plain of Rome. I look upon this as a very important fact. To me it clears up what might otherwise be a very great mystery. We very often find in an outbreak of Splenic Fever we can trace no contagion whatever. There has been no Splenic Fever

in that district within the memory of man. How has it come there? If we bear in mind that these bacilli may at one time be virulent and another time not virulent, I think we can clear up the mystery. A bacillus is developed during certain changes in hay tea and brewers' grains. This bacillus to all appearance is the same as the bacillus of Splenic Fever, but the bacillus of hay-tea has no virulent effect—in fact, animals are eating every day of their lives millions of these very bacilli. But some German experimenters have proved to demonstration that this innocent bacillus will, under certain conditions, become virulent. By cultivation in blood serum it attains all the virulence of the Bacillus anthracis, and then inoculation with cultivated bacilli will produce all the symptoms of death from Splenic Fever. You have here, I think, a key to the fact of the so called spontaneous outbreak of this disease. Owing to some peculiar condition of fermentation the bacillus of hay-tea and cotton cakes, and so on, will become altered, and what is the consequence? Very often you will find that the feeding of the animals upon the impaired cotton cake will induce Splenic Fever. How has this bacillus got there? Simply owing, in my opinion, to the transformation of the innocuous Bacillus subtilis into the virulent Bacillus anthracis, owing to its having propagated for generation after generation in this cake. I think a recognition of these facts will enable us to throw a light upon the so-called spontaneous eruption of these diseases. That a change takes place is proved by the cultivations of Pasteur. It is easy to cultivate downwards, and by cultivating downwards the Bacillus anthracis becomes almost innocuous, and the facts which have been established by Pasteur himself are sufficient to prove to any impartial mind that by attenuating the virus you alter the character altogether. It loses its virulence. Cultivate it again within an animal body, it regains its virulence. Hence we have a key to the outbreak of so-called contagious diseases in this country. I do think, of course, these organisms die under certain conditions; under conditions they will live for ages. One writer a short time ago—I think it was in *Longman's Magazine*—has applied these facts to the theory of universal life from the beginning to the end; and he has construed these facts as an evidence of universal life. The Bacillus anthracis if kept under certain conditions is a virulent feeder of disease, but when taken from the body of the animal and it comes into conditions which are not nutritive, it will die. It is a most difficult matter to extricate them. I only wanted to draw attention to the fact that we have always amongst us certain bacilli, which, under certain conditions, may become pathogenic, and in this way give rise to disease. I will illustrate this by one circumstance. Some three years ago I was called to a place in Scotland where five cows, two horses, twelve or eighteen pigs, and several cats had died from Anthrax between Sunday night and Tuesday morning. All the symptoms of Splenic Fever were discovered, and bacilli in great numbers. What was the cause of this? These animals had all been eating the same food and drinking the same water for a long period of time. There was nothing to bring the contagion: what was the source of it? That was a difficult matter. I may say that there had been no new stock arrived on the place for a very long period of time. On examining the water I found it certainly not at all nice-looking, and there was a slight smell about it, and on examining the matter carefully I found that the water was obtained from a common reservoir or pipe, that all the animals partook of—the cats, the pigs, the horses, and the cows. The horses were fed upon one kind of food, the cows upon another. Very well; on investigating the source of the contamination of this water, I found the water itself came from a very nice clean pond with a really beautiful spring of water, as nice as could be. But during its course in common red drain pipes I found that a manure heap had been lying upon the course of this drain for about a twelvemonth or more. The manure heap had been removed

by the time I was there, and there was nothing to be seen—I elicited the facts by inquiry. On examining this water nothing could be found microscopically, but on making cultivations in various fluids and inoculating with the water the bacillus was developed as beautifully as it was found in the spleen of these animals. I concluded then that the bacilli were contained in the water. The water was changed there and then, and there was not another case on the place. How did the bacilli get into the water? That was the question to solve.

A MEMBER: Where were the other animals that did not die? What did they drink?

Professor WILLIAMS: They were there. I think that was an illogical question. We hear of animals subjected to the same conditions; some succumb rapidly, some after a longer period of time, some not at all—I am surprised at the question. The hay-tea bacilli contained in the hay-tea of the manure were saturated into the soil from the manure heap, as was proved afterwards on thoroughly cleaning the drain out and having new pipes laid down; the hay-tea manure had bred brood after brood, or generation after generation, in the soil surrounding these pipes, and during the course of those generations they became virulent; and that virulence was sufficient to induce this disease. We run about with the idea that contagious disease must be imported. I think by experiments of this kind we can deduce from the facts brought before us, that it is a fact that the virulence of the breed does increase under certain favourable conditions. Then again, I heard Professor Axe say that specific disease was not found over a very wide area, it spread from place to place; but unfortunately for himself he mentioned Influenza as an instance of specific disease. Now, I think our experience has led us to conclude—in fact, I know, that Influenza will break out over an immense tract of country in the course of a few hours, scores and hundreds of miles apart. In Chicago, and in New York, and in various parts of Canada, it was apparent in the course of a few hours; and not only there, but on an island in the Hudson river, where there was only one horse. Therefore, these bacteria must have, owing to some circumstances surrounding them, a virulence sufficient to induce the disease in animals subjected to their influence.

Mr. J. B. WOLSTENHOLME: The task of constructing a paper is much more difficult than that of criticising. My idea of the lines on which such a paper would have run would be—Origin of Diseases, Origin of Species. From what the learned gentleman, the writer of this paper, says, "It will probably be admitted that each seed, or herb bearing seed after its kind, is an aboriginal creation." I should gather he is of opinion that each species of animal is also a kind of aboriginal creation. Of course that is not actually in harmony with modern thought and modern theory; to accept the premises and the deductions of Darwin would lead us altogether into another field. But without referring to the vegetable kingdom, which Sir Frederick has done, in my mind from what we know of the fauna and flora of the geological age it is entirely different from that of to-day; and if we also look to the same age for the different species of animals compared with what we have to-day, we shall find they are altogether different from what we had then. If we admit the theory of evolution, and of the change of mode and action and condition in different periods, I think that we should also study carefully the influence of such changed conditions as applicable to disease. And I should think that the conditions of life of the early Britons, as compared with those of to-day, are just as diverse as those of the pre-Adamite creation from the present conditions of life. I look at the conditions of life in our manufacturing towns, and the work in such places as cotton mills and foundries, and I compare that with the conditions of life found in this country when Julius Cæsar arrived here, and I cannot but think that those diverse conditions, spread

over a period of time, would have some modifying influence upon the different forms of disease. It is not for me to say that they will produce new diseases; but I should myself infer that they would call forth new forms of disease. For my own part, I should have been greatly interested if this subject had been taken up, for I believe that alteration of conditions in the human subject and in our own patients has a great deal to do with the bringing forth of different diseases, and I think an appreciation of this fact would lead to extended observation and, perhaps, more accurate treatment.

Professor WALLEY: I am not going to find much fault with the paper. I know sometimes in this kind of paper, it is a great advantage that it leaves a few blanks for you to fill up by merely introducing the subject, and I suppose by some that is thought to be all that is required in a paper of this kind. Certainly if you look over the paper you do not get a vast deal of information from it, and in an important subject of this kind, especially as a major subject, I should certainly have looked to have seen a greater degree of elaboration. We must, however, take it for what it is worth. There are a few things I should like to direct attention to in the paper, and the remarks made by the speakers. In the first paragraph there is a statement made, that the only specific diseases affecting the horse are Influenza, and Glanders, and Farcy. Now, as Professor Axe has pointed out, that is quite beside the fact; you have got not only Anthrax but a special form of venereal disease, and the horse is liable to Rabies although it is not indigenous to the horse. He goes on to say that Ophthalmia is not a specific disease. I am glad to see this remark in print, because it is held by some responsible members of our profession that it is specific, and it is a good thing that he has been able to contradict this assertion. There is nothing at all specific about the so-called specific Ophthalmia of the horse—not that that has much to do with the subject under discussion. In the third paragraph two questions are put: first, "What is the germ of each specific disease?" and the answer given is "We do not know." Gentlemen, if we do not know anything about the germs of specific disease, why talk about them? I hold that we have discovered something about the germs of specific disease. We can certainly, as Professor Axe has pointed out, handle them and cultivate them; we can see them; and what is more we can watch their growth; so that we do know a little about them. Certainly, the differences are in some cases very slight indeed—mere morphological differences, as far as the germs are concerned; nevertheless, we can differentiate them one from the other. In the fifth paragraph a statement is made, that it is not possible or probable that we shall ever get rid of some of these germs; that the germs of those specific diseases which have their habitat in this country will never die out. I think that is too pessimistic a view to be taken of this subject altogether. That they do not die out I think is hardly proved; that we do get rid of the diseases, I think is well proved. I need not refer to the outbreak of Rabies which occurred in Edinburgh some years ago; we have had no case now for four or five years, except one on the south side. With reference to Glanders, so far as I know there is not a single glandered horse in the city of Edinburgh, and in no place could it at one time have raged with greater violence. I do not say the germs of these diseases are actually killed. We may have destroyed them entirely in some cases; but I say if they have not been killed, the growth has been prevented by want of a proper dwelling, and I maintain there is no disease that cannot be got rid of amongst animals, if proper measures be adopted. If a disease breaks out amongst human beings, you cannot poleaxe or shoot the individuals infected or attacked by it, and in this respect they are scarcely upon a par with animals. We have not to face an uncle or an aunt, who would say to us, "What have you killed my relative for?" and I maintain that we do get rid of these diseases in this rough-and-ready, but very effectual

manner. With reference to the vitality of the virus, I think that the vitality of some of the virus is doubtless very prolonged. I know we have ample evidence that the vitality of the virus of the Cattle Plague may be retained for months and even for two years in the graves of animals who have died of the disease. The same is proved with reference to Anthrax and Swine Fever, and the same with reference to Foot-and-mouth Disease. You will see that the virus retains its vitality for six or twelve months, or probably longer, in the walls of old buildings. I do not know why any one of these germs cannot or may not live for a very extended period. Now Professor Williams and Mr. Wolstenholme indirectly referred to the evolution of germs. This is a matter on which I have expressed my own opinions in writing on several occasions, and I am quite satisfied that under particular circumstances non-pathogenic develop into pathogenic germs. I am not an ardent believer in the evolution theory as far as man is concerned, but we know that plants and the lower animals undergo very important morphogenic changes, and I am quite convinced that some non-pathogenic germs do, under special circumstances, develop pathogenic properties. You will find that examined microscopically they bear an intimate resemblance to each other, which can scarcely be differentiated, and I do not see that we are stretching a great point in assuming that some of these germs do under special circumstances contract pathogenic properties. One thing, in relation to this matter, seems to be lost sight of. It is generally believed, I think, that a great part of the power of these germs to produce death is owing to their power of producing ferments. Now these germs must be existing under conditions where it is utterly impossible for them to produce these ferments, but so soon as this condition is altered, then they begin, no doubt, to develop virulent and destructive properties. Professor Williams has drawn attention to cases of death by the Anthrax bacillus in animals fed upon cakes, and I have myself had a number of cases of death amongst animals actually from Anthrax, and in all those cases in which I could not trace it to the other sources, I have found the bacillus in the cake. How does it get into them? We know the spores of Bacillus anthracis live for months in soil, and water, and so on. My own impression is, these spores are present in the cake, and that they are introduced there by the vegetable matter, of which the cakes are made, having grown over anthrax graves. In relation to water, the matter is of the utmost importance, in regard to the possibility of its conveying the Anthrax bacillus, or rather the spores of them. I had a similar case to that mentioned by Professor Williams, and in that case I found that the dead body of an animal, evidently suffering from Anthrax, had been buried in the soil through which the water ran. There is, no doubt, I think under special circumstances these germs do develop pathogenic properties, and I think one thing with reference to the vitality of the germs is the power which the spores retain of germinating after lying dormant in the earth for an indefinite period. There is a remark in the last paragraph with reference to home specific diseases. There are some who believe that under special circumstances colds—catarrh—will develop infectious properties not only amongst animals but amongst men, and for myself I am positively of opinion that Pneumonia does develop specific properties in the horse; and I say this from a not very limited amount of observation. There are many other things which might be touched upon. I hope, however, that the meeting will not pass without an expression of opinion upon the matter of the absurdity of talking about our inability to suppress these diseases. As I have said before, we can suppress and have suppressed some of the most virulent diseases to which animals are subject, and if we were only to adopt proper treatment and proper measures, with reference to the Privy Council, I maintain there is not a single specific disease affecting animals in the British Islands that could not be got rid of within five years.

Mr. A. ROBINSON: The service of mares very frequently brings about pathological conditions, and probably other animals will be affected in the same way. And there is another thing, with regard to the development of diseases, overlooked. It has been proved in many instances that if a mother is affected with disease while bearing her offspring that her being affected gives immunity to the offspring. On the other hand, it not unfrequently happens that a mare may recover from Strangles a fortnight or three weeks before foaling, and about a month after she had foaled, the foal will be very bad with Strangles, showing that in all probability the mother had communicated the disease to the foal.

Mr. HOPKIN: The paper and its name is somewhat misleading. Specific diseases, as defined by the essayist, depend largely on their mode of propagation. To illustrate this, let me ask you to consider how Rabies becomes what our essayist calls specific. Simply, I believe, due to a rabid dog going wandering about the country and biting other dogs. This peculiar development does not take place in other animals, or if it does, they are under restraint; and thus our essayist would no doubt class Rabies as a specific disease of the dog. The study of the mode of propagation is most essential, and quite equal in value to the study of the origin. Possibly, the origin may be difficult to get at, and we shall have to direct our attention more to the way disease is developed and propagated. In speaking of the propagation, we have in Manchester several large studs of horses, and I believe more or less Glanders is always present, and from these centres we can occasionally trace other outbreaks of Glanders. Our local authorities take no action, and poor people, who are generally the purchasers of cast-off horses, become the losers. In a case in my own practice a poor man lost six horses from Glanders contracted in this way.

Mr. LOCKE: I cannot allow Mr. Hopkin to make these remarks with regard to Manchester. It is altogether wrong.

Mr. HOPKIN: It is not necessary to ask Mr. Locke's permission when I am stating facts. I am in such a position that I can prove my statement by means of dates and full particulars.

Professor WILLIAMS: This is a paper on the origin of disease.

Mr. HOPKIN: We have not been discussing the origin, but the propagation of specific diseases. What I wish to say with regard to this subject is that, when not latent in the system of the animal, it often becomes localised in the buildings. The floors, the roof, the walls, the woodwork—they may all become impregnated with the virus. Cement and non-porous substances should be used in all dangerous studs. Men and animals become acclimatised to diseases, and it is often when the system undergoes some change that a latent disease develops itself. The sudden spread of Influenza over large districts—I am not clear as to this being spread from one animal to another, but rather lean to the belief that it is due to atmospherical influences over large areas.

Mr. GREAVES: I have listened with great pleasure to the remarks made by Professor Axe, Professor Williams, and others. I do not think they have controverted any fundamental point Sir Frederick has put forward; they have dilated upon them, and at that I am not much surprised. The discussion has not turned upon the point which I hoped it would have done; we have been talking about germs which were not known in our forefathers' time. I should have liked to have known what diseases Adam and his sons and his family died of. Men lived then to eight hundred or nine hundred years, but there were also plenty who died when they were infants. What did they die of? Were there more germs then? I think they must have been the same diseases that kill people now. There is another thing I wish to say before I sit down. I cannot allow it to pass that Manchester is a seat of

Glanders, to any great extent. I have been practising in Manchester fifty years, in some of the largest establishments, and have not known Glanders once in fifty years in those establishments. It may be in an establishment I know nothing about; it does not belong to the large establishments I have to do with. We are all indebted very much to Professor Axe for dilating in such an eloquent manner on this subject, fraught with many difficulties far from being solved yet; but I hope that in course of time these difficulties will be solved by science, and that before many years are over we shall see these diseases stamped out. But what becomes of the veterinary surgeon then? When all these diseases are stamped out, and there is no disease to occupy us, what profession must we take to?

Mr. BANHAM: I think, gentlemen, we ought not to find fault with the gentlemen who come forward to write papers for our meetings; if we do, we shall find that very few will desire to write papers for us to discuss. This paper is put forward, I believe, in all good faith, and if it is not to your satisfaction, then it is for you to make it so. Our friend Professor Axe has taken up the subject as well as we could possibly wish, and I think most of the speakers that followed him have done likewise. Now that I am on my legs, I might just as well say one or two words with regard to the evolution of these organisms. It is quite possible that some of these organisms may change their character, but we must consider that we do not at present know what the effects of these organisms are upon the system; it is quite possible that we may mistake one organism for another—they are so very much alike, and we have no test for some of them. On the mode of cultivation of these organisms, too, I may say that they differ and vary under certain conditions considerably. Take, for instance, the Anthrax bacillus, which is, I believe, the best known to us at the present time. In its cultivation, there are stages at which an ordinary observer would think that he had a different organism to deal with altogether, and it is only when this organism is put under certain conditions, that it will take the same shape and form it originally started with. Then, again, it is possible that these organisms may kill, or that the animal may not succumb to the organism itself, but to some ferment which has been produced by it. It may be possible that an organism may be dangerous at one stage and not at another, and if so it will ratify the remarks of Professor Williams with reference to some of the organisms he has investigated himself.

Mr. ROBINSON: I think it is generally accepted that one organism never does change into another. The power of an organism may be increased or may be reduced, but never may it be altered to another organism; and one can hardly understand the contention of the speakers who have put forward that theory. You can increase the power of an organism, but you can never change it. Certainly, it has different stages of life, as all animals have, but it certainly has never developed from a monkey to a man.

Prof. AXE: Mr. President, I think there is but one point arising out of the discussion of this subject to which I need direct your attention. It has been suggested to you that micro-organisms of a simple septic character have the power, under certain special conditions, of acquiring pathogenic or disease-producing properties—that is to say, an organism by nature perfectly innocuous may undergo such a change in its physiological properties as to be capable of living in an animal body, where it could not live before, and of exciting there a specific infectious disease. Now, the grounds on which this proposition has been put forward are two, one of which has reference to the circumstances relating to the propagation of Roman Fever, the other to the reputed relation of the common hay bacillus to Anthrax. Professor Williams observes that in the marshes of the Roman plain there is constantly eliminated from the ground a microbe having the effect of producing fever in

man, and that when the tainted blood of the human subject is transmitted to the dog that animal will contract the disease. I do not exactly appreciate the relevance of this fact to the question in point, unless it is intended to imply that the virus while in the human body has contracted new properties. To show this it would seem necessary to be assured that the dog is not receptive of the original miasmatic microbe, and, nextly, that the particular species of microbe implanted in the man did also exist in the dog and none other, and that the disease could be transmitted from one infected dog to a healthy one. It is further stated that when persons who have inhabited these miasmatic localities leave them, to occupy insanitary dwellings elsewhere, they then acquire the power of transmitting the disease to others—a power, it is said, not before possessed by them; and the Professor suggests that these original miasmatic organisms under the insanitary conditions acquire new properties. Admitting the facts to be as stated, I think they are capable of an altogether different interpretation—viz., that the insanitary state referred to as acting upon the organism in the human bearer may possibly have merely rendered receptive persons who, under better conditions of life, resist the infectant. The other facts on which Professor Williams relies for proof of this strange physiological change are in his own experience of Anthrax, and, indeed, I might say, a similar experience is familiar to many of you. They are briefly these. Splenic Fever appeared upon a farm where it had not been known before, and inquiry failed to trace the virus to any extraneous source. A manure heap is found contiguous with a drain, from which water is taken by the stricken animals. What is the inference we are asked to draw from these facts? Why, that the harmless bacilli originally contained in the hay now forming part of the manure acquired pathogenic properties, and produced this disease, Anthrax. To justify such an inference it would require that this particular manure heap and this particular watercourse differed in some very obvious fundamental particular from the many thousands of watercourses and manure heaps, which everywhere abound in the closest proximity to each other, where Anthrax never was known. But let me test the logic of the case referred to, by one in my own experience. At Stoke-under-Ham, in Somersetshire, an outbreak of Anthrax occurred on a farm where the disease had not previously been known to exist. Inquiry showed that a horse had died suddenly, several weeks past, and had been opened on the manure in the crew-yard. Contiguous with the yard, and sloping down from it, was a pond, out of which the farm horses usually obtained their water. At the time of this mysterious death the weather was dry, and it continued so for several weeks afterwards, so that no drainage occurred during this time between the yard and the pond. Soon, however, rain fell in considerable amount, and one after the other all the horses which drank from the pond died. The blood and virus of the dead beast, having been washed out of the manure into the water, was consumed by the horses, with the result I have stated. Now, I have simply to ask, is it not more consistent with all we know of manure heaps and watercourses to conclude that some such direct contamination occurred in the case related by Professor Williams than that the hay bacillus affected the physiological singularity attributed to it. This question of the acquirement of pathological properties by innocent microbes originated, I believe, with Buchner, who has the reputation of having converted the bacillus of hay infusion into the bacillus of Anthrax. Morphologically, these two bacilli are very distinct, and both Klein and Koch regard the experiments of Buchner as altogether unreliable. Both these able bacteriologists have failed to confirm the results which Buchner says he has obtained. Now, I do not mean to affirm that innocent microphytes cannot acquire pathogenic properties, but I do think that we are not justified by anything at present known in accepting such a

doctrine. It is quite evident that by scientific methods of cultivation they are capable of great modification in the intensity of their action on the living organism—of being rendered less virulent, and converted into a protective "vaccine"—but beyond this we have no reliable evidence to guide us in the direction indicated.

Mr. HUNTING: I propose that we should pass a resolution showing the sense of the meeting. I therefore move, "That this meeting is of opinion that the contagious diseases of animals do not arise spontaneously; that they are spread solely by contagion, and may be effectually stamped out by the adoption of proper measures."

Professor WALLEY: Excuse me, I think a resolution very similar was put last year.

Mr. STANLEY: I do not see what it has to do with the paper that has been read. I do not think we are called upon to pass such a resolution at all.

The PRESIDENT: Before I put this, I would ask you whether it is acceptable. I will ask those who are in favour of the resolution being put to hold up a hand.

Mr. WRAGG: I shall be pleased to second it, to test the sense of the meeting.

The PRESIDENT: I simply put it as it stands. I ask those who are in favour of its being put to hold up a hand.

Mr. HUNTING: Perhaps somebody would like to propose an amendment negativing that.

Professor AXE: I really think the proposition is a very innocent one. If we adopt it, it may be the means of emphasising what has been already expressed. If it does nothing more, it will certainly do that, and if we reject it we place ourselves in a rather awkward position. I do not really see that there is any objection to it.

The PRESIDENT: I do not think there is any necessity for discussing this question; it is simply a question for vote.

The resolution was put to the meeting and carried unanimously.

Mr. HOPKIN: I have asked the permission of the President to make a statement in confirmation of the facts to which Mr. Locke took exception. Mr. Bell tells me a client of his purchased two horses from an establishment in Manchester, which horses have infected his stud with Glanders, and he has lost the whole of it. I have also the pleasing duty of moving a vote of thanks to Sir F. Fitzwygram for his paper; though we have had some adverse criticism, we all, nevertheless, owe him a debt of gratitude. He has done much for us as a profession, and, as he so nicely suggested in his speech last night, he is the first member of our profession that has ever sat in the British House of Commons. I am certain we shall all heartily thank him for this last effort to be of benefit to us by the writing of the paper to which I have referred.

Captain RUSSELL: I have much pleasure in seconding the proposition.

The PRESIDENT: I scarcely think it necessary to put this proposition to the meeting. Sir F. Fitzwygram's paper has given rise to the best discussion we have had at this meeting, and I am sure you will agree to the resolution that a vote of thanks be passed.

Carried by acclamation.

Professor WALLEY: I have a pleasant duty to perform—that is, to propose a vote of thanks to the gentleman who opened this discussion, namely, Professor Axe. Had it not been for his remarks upon the paper, I think the discussion would have been a very poor one. I do not agree with all he has said; at the same time, he has given us an opportunity of ventilating our opinions.

Mr. BLAKEWAY seconded the proposition, which was carried unanimously.

Professor AXE: I can only say in return, if it has afforded you as much pleasure to listen to my remarks as it has given me to make them, you have spent a most enjoyable morning indeed.

Mr. ROBINSON proposed a vote of thanks to Mr. Rutherford for opening the discussion of yesterday.

The PRESIDENT: I should be sorry that Mr. Rutherford should think there was anything like neglect on my part, or want of desire on yours, to express our obligation to him for what he said and what he did yesterday. The whole of these resolutions should have come on at the end of the meeting. Will the meeting, now that it has gone so far, allow me to take up what Mr. Robinson has proposed? I am sure the information given by Mr. Rutherford was second to none given to this meeting. For myself, I have learned perhaps more from Mr. Rutherford's remarks of yesterday than I have on any one day that I have lived. I have great pleasure in seconding the vote of thanks.

Carried unanimously.

Mr. RUTHERFORD: I am obliged indeed for the cordial manner in which you have thanked me. If at any time I can supplement those remarks more to your advantage, I am sure I will be your humble servant.

Obituary.

THE death is reported of Frank Garratt, M.R.C.V.S., Veterinary Surgeon, First Class, Army Veterinary Department. Mr. Garratt graduated in 1870, and soon after joined the Army. He died at Meerut, India, somewhat suddenly on September 16th, leaving a young widow and two children. During his army service he was a great favourite with the different corps in which he served.

D. E. James, M.R.C.V.S., died at his residence, Bridge House, Haverfordwest, on the 15th October; universally regretted throughout the country. He graduated in 1858.

A name long familiar to those members of the profession who graduated in Scotland until a few years ago, has disappeared by the demise, in Edinburgh, of James Dunsmure, M.D., F.R.S.E., F.R.C.S.E. This gentleman was for many years Secretary of the Scottish Section of the Examining Board of the Royal College, as well as a Member of the Board. During his long connection with the profession, he rendered it yeoman's service by the faithful and courteous manner in which he discharged the somewhat responsible and onerous duties of his office. Dying full of years, and honoured by the citizens of Edinburgh, we think it our duty to record the debt we owe him for the kindly assistance he gave the profession during days of trouble and distraction in Scotland.

Correspondence.

WHY NOT JOIN THE NATIONAL VETERINARY BENEVOLENT SOCIETY?

SIR,—Do we not all know that life is uncertain; that in the performance of the duties appertaining to our profession, dangers surround us on every side; that in a moment we may meet with an accident that will call us to that bourn from whence no traveller returns? If such a one happens to have a wife and family unprovided for, how deplorable, and with what vain and terrible regrets his mind (if conscious at the time) must be tortured in

its last moments, knowing that nothing but penury, want, and misery are in store for those whom he is now leaving behind him, and who were nearest and dearest to him in the world! What awful deathbed reflections these must be to a sensitive and honourable man, especially when he remembers that there is a National Veterinary Benevolent Society existing, able and willing to be a friend to the friendless, and a father to the fatherless.

These reflections have occurred to my mind on receiving a letter, dated September 29th last, from Mr. Thomas Clarke, of Huntingdon, informing me of the sudden and melancholy death of Mr. George Fowler, veterinary surgeon, of that town, whose death was the result of a violent injury sustained whilst galloping a horse in a gentleman's park to test its lameness. It appears the horse slightly stumbled, swerved, and plunged under a tree, one of the branches of which struck him across the mouth. He was hurled forcibly to the ground, became unconscious, and died as the doctor arrived. This was just such a melancholy example as happened to Mr. William Field, junr., London, while hunting a few years ago. But why confine myself to these two cases? there are many others, equally sudden and deplorable, happening to our members in the prime of life, whilst in the performance of their professional duties, when everything looks bright and fair, and at a moment when casualties and death are never thought of. Under these circumstances is it not wise, nay, is it not the bounden duty of every prudent veterinary surgeon who has a wife and children whom he loves, to make provision for them?

In this case, Mrs. Fowler is left with four or five children in need of help, and although he was a veterinary surgeon, he was not a member of our society. Our rules preclude granting her help from its funds, but we can and will help her from our private purse, and I hope, and beg, that every one who reads this will lend a helping hand to her in her hour of sorrow and suffering.

Remember, duty begins with life and ends with death; it compasses our whole life. *Let us do it*, till the day dawn and shadows flee away.

Manchester, *Oct. 11th*, 1886. T. GREAVES, F.R.C.V.S.

INOCULATION IN PLEURO-PNEUMONIA.

SIR,—The recent outbreak of Pleuro-pneumonia in Kirkcudbrightshire, and particularly the successful measures adopted for its suppression by Mr. Rutherford, the well-known specialist in inoculation, have provoked Mr. Campbell, F.R.C.V.S., to initiate a discussion in the *Kirkcudbrightshire Advertiser*, that has been somewhat bitter and prolonged. If you will favour me with space I shall make some remarks on the discussion.

It appears that the outbreak occurred on the 8th July, and that, soon after this date, Mr. Rutherford inoculated the healthy animals of the infected herd. Mr. Campbell, evidently displeased at Mr. Rutherford's appearance in the county, and not a little envious of his success in suppressing Contagious Pleuro-pneumonia, sent a long letter to the local paper of the 17th September. In one view that letter was intended to enlighten the members of the Stewartry Local Authority, as it contained "a few observations regarding the suppressive measures likely to be recommended by them"; but in another view, and obviously the correct one—as the Local Authority had already taken measures—the letter was intended to shake their confidence in Mr. Rutherford's method of checking the progress of the disease. The Local Authority met three days after the publication of Mr. Campbell's letter. That the letter succeeded in its motive to a small extent is made clear by the discussion that took place at the meeting. Mr. Campbell attended, and in reply to the Chairman said:—"He would strongly recommend the Local Authority, if there was not in their county a man competent to be a veterinary inspector, to look out

for such a man and appoint him as soon as possible. It was the duty of every inspector to make himself conversant with scientific discoveries and research, and with current literature, and to keep his Local Authority posted up in these subjects. Inoculation was not a new discovery; he was an advocate of it in 1865, and performed it at Barcloy, in this county. Since then experiments had been made in Hungary and elsewhere, and it had been found that inoculated animals had contaminated healthy animals. He would advise the Local Authority not to inoculate, but if they decided otherwise he was as competent to do it as any man in Scotland. If he were not so he would not be a member of the Council of the Royal College of Veterinary Surgeons."

That is a nice speech for a member of Council. He advises the Local Authority to appoint a man, and then spouts his own qualifications for the office. How painfully ignorant of Mr. Rutherford! Mr. Campbell's statement of his own competence to inoculate is an empty assertion. Where are the particulars? Mr. Campbell was not sent to the Council because he could inoculate. It grieves me to think that he can be so stupid. Pity it is that he was sent at all. His professional attainments were not thought of at the election.

Mr. Campbell's later conduct at the meeting referred to deserves the strongest condemnation. When a member of the Local Authority assailed Mr. Rutherford's reputation in disparaging terms, Mr. Campbell opened not his mouth. As a member of Council, if he chose, as a fellow-practitioner, he was bound in honour to stand up for an absent and unoffending professional brother. I am certain that Mr. Rutherford would not have remained silent if Mr. Campbell had been the subject of the insult.

Other letters followed Mr. Campbell's, and so the correspondence continued. I regret very much that the writers did not change the *venue*, and give your readers the satisfaction of perusing a series of rather warm letters. Besides, such discussions in the lay press are only fruitful of harm to the profession.

Mr. Rutherford writes from experience with precision, states facts, and carries conviction with him. His arguments are well supported by Mr. Cunningham, who gives statistics from his own practice. Mr. Campbell writes much from his imagination, and his knowledge of things as he would wish them to be; he quotes largely from Dr. Fleming's "Sanitary Science and Police" and from Professor Walley's "Scourges." But Walley confesses that he was a neophyte when his attention was first directed to inoculation. It may be doubted if his higher parts have much altered since then. Mr. Campbell says he has had experience of inoculation—but not much. It was in 1865 at Barcloy; the result he takes care not to mention. He, too, would be a neophyte then. He gives no facts within his own ken, yet he has the audacity to condemn inoculation without knowing anything about it. What caused the howl of indignation at Mr. Rutherford's advent into your county, Mr. Campbell? Was the prospect of the shekels passing to Edinburgh too much for your philosophy?

Mr. Campbell should think twice before he speaks once; and, for a time—a long time—he should not write at all. To clear his intellect, I would advise him to read and study the following by Lord Kames:—

"When reason acquires such strength as to stand on its own bottom without the aid of authority, or even in opposition to authority, this may be called its manly age. But in most men, it hardly ever arrives at this period. Many by their situation in life have not the opportunity of cultivating their rational power. Many from the habit they have acquired of submitting their opinions to the authority of others, or from some other principle which operates more powerfully than the love of truth, suffer their judgment to be carried along to the end of their days, either by the authority of a leader, or of a party, or of the multitude, or by their own passions. Such persons, however acute, may be

said to be all their days children in understanding. They reason, they dispute, and perhaps write, but it is not that they may find the truth, but that they may defend opinions which have descended to them by inheritance, or into which they have fallen by accident or been led by affection."

Apologizing for the length of this letter, PETER RAMUS.

"TIPPING."

DEAR SIR,—I am very pleased Mr. Fraser has brought the subject of "tipping" before your readers again; but I am sorry he has attempted to justify the "custom." With your permission, I will endeavour to answer his remarks *seriatim*. Why the truth should not be "allowed to pass without protest" I cannot understand, except on the ground that the "truth is not always pleasant." He next finds fault with my "bringing such monstrous charges against honest men." I fail to see that I have been guilty of such an offence. At any rate, he has only to make enquiries amongst practitioners of veterinary medicine, farriers, and tradesmen in the West-end of London, and he will probably get the "proof" he seems to think so necessary for my conduct.

It is "very strange" to me (as Mr. Fraser puts it) that he has never been asked by grooms what percentage he gives on his bills. The only explanations I can offer for his consideration are: 1st. Perhaps he has never had a shoeing forge, and the custom may be more prevalent in that branch of trade, although I could give him instances in *bona fide* practice. 2nd. It may not have been necessary in his case, as satisfaction could be obtained without asking. 3rd. That all the grooms he has come in contact with have been of the best type. 4th. That he has always been fortunate enough to practise in districts where the custom has not reached. That I am not "mistaken or misinformed" I am certain, as I have unfortunately been the culprit myself, and have lost more than one client simply and solely because I refused to comply with such demands.

I not only said in my address that the "tradesman (veterinary practitioner included) puts his hand in his client's pocket to pay the servant," but I went even further, and stated the fact plainer and more forcible, by the truth thus: "*the servant uses the tradesman to rob his master.*" And I would ask Mr. Fraser to tell me what else it is?

In passing I will ask Mr. Fraser to allow me to point out that I draw no invidious distinctions between classes, one to my mind being equal to another in the social scale of life when "*honesty*" is in question.

Mr. Fraser then goes on to try and justify such "customs." What "extra work the servant" of our client has to do with us I have yet to learn. Further, he says, "if we are to cure our patients quickly, we must have the sympathy of the groom or cowman," which "*can be secured only by a gratuity.* If such practices as these are given as reasons why the "custom of tipping" should not be questioned, I must still oppose and cry down the "custom" with tenfold energy. In fact, it is a shameful confession, and shows up the character of the groom and cowman, and the position of the veterinary practitioner, in plainer language than any sentence in my address. I would no more "promise a gratuity," even "in the presence of my client" (unless I wished to insult him), than I would receive one from a dealer, even if my client were present.

If the servants of our clients are not permitted to wait upon us, and carry out our instructions as nurses do for human practitioners, the sooner we endeavour to educate our employers to the necessity for it the better, instead of playing into the hands of the servants themselves. I would suggest that it is *our duty* to endeavour to train the mind of the groom to do all in his power to "nurse" his employer's animal, and not only for our own reputation, but to relieve the sufferings of a helpless brute. What, however, is more important to the servant, is to show his own value to his employer; and to keep a

situation to earn a livelihood (a notion which, I fear, rarely enters the minds of so-called working-men at the present day). Rather than "tip" grooms to do their "*duty*" I would impress upon them (if possible) their "*responsibility*" in this life.

If we are supposed to do all the little "details" Mr. Fraser seems to think is "*our duty*," we should not require to see patients each day, or I am afraid some would get neglected. I may say, however, so far as I am personally concerned, I do as many of these "details" as I possibly can, and it would do many of us good to do more, for I blush to confess that some of us are not perfect at even placing a bandage on a horse's leg, and doubtless there are some who would rather "tip" a groom than expose their ignorance of such "details."

In conclusion, Mr. Editor, allow me to impress upon Mr. Fraser and your readers, that the only object I had in bringing these facts before the public, was to expose and condemn the custom; and I am pleased to learn that Mr. Fraser will give his help to try and abolish it, and trust others will follow suit, although they will find it a difficult task. The reason I spoke out about the members of the veterinary profession, was because I happen to be better acquainted with them than any others, and the assembly to which it was addressed is brought in contact with us, and therefore likely to be impressed with it. As far as singling out our profession as the only culprit is concerned, the word "tradesman" I think is broad enough to embrace others, according to Mr. Fraser's own ideas.

Whether your correspondent wishes to infer that your humble servant has not had experience or not, I do not know; but if my asservation is of any use to convince him, I can confidently tell him that there is not a word in the address he seems so much to complain of, which has not been experienced by myself; and if he requires any further "proof," I would recommend him to commence practice in the West-end of London, and give the profession his experience a year or so hence.

I must apologise for taking up so much of your space, and take this opportunity of thanking you for the courteous manner in which you publish your correspondents' views on the various topics of veterinary science.

Finally, I beg to thank Mr. Fraser for giving me this opportunity of dilating upon the views I expressed at Norwich, and I hope it will convince him of their truthfulness; and that he shows in his own writing that similar "customs" exist, and also that wherever he has attempted to justify them, he has failed.

Downing Street, Cambridge. GEO. A. BANHAM.

DEAR SIR,—Having read the letters in the VETERINARY JOURNAL on the subject of "Tipping," etc., I am sorry to say that my experience leads me to think that it is carried on in some places—at least, in the neighbourhood where I reside, although a varied experience in different parts of the country does not impress me that it is the general rule. I believe that incompetent men sometimes have resource to tipping grooms and favouring dealers, with the object of getting custom. It is known to many, that dealers have their favourite veterinary surgeon, who is generally a shaky gentleman, but, worse luck! often falls in for good clients through the dealer's recommendation. My services are not often required by dealers in the examination of horses for soundness, as I understand a clean certificate can be had from So-and-so without much trouble. I am one of that class who keep dealers in their places, and consequently seldom have a good word from them. Cases of misrepresentation are seldom brought to light; the dealer knows better than to go into court; if he sees the buyer really means to make a lawsuit of it, he will endeavour to make an exchange or allow the animal to be returned. As an

antidote to this sort or trading, I should recommend, that where a suitable case is afforded, both dealer and veterinary surgeon should be made an example of, and the name of the latter be struck off the Register.

Mistakes in examinations may be made sometimes by the best-intentioned man, but in cases of doubt, let his character and previous business transactions be investigated. Yours truly, "HONESTAS."

DEAR SIR,—Will you have the kindness to grant me a small space in your Journal in answer to Mr. Fraser as to "tipping"? In the remarks made by Mr. Banham at the E.C.V.M. Association, I cannot see what Mr. Fraser has to protest against. I have no doubt he is an honourable man. We are all honourable men; but, unfortunately, we are not always brought in contact with such in our daily round of duties. What I consider Mr. Banham in a very matter-of-fact way did, was to draw the attention of those present to the difficulty that the young practitioner had to contend with in the system of tipping those who were in charge of the animals he might be called on to attend; and not the young alone, for I think the practice must be *very* select, and the practitioner very fortunate who has not had to face this nuisance.

I do not draw from Mr. Banham's remarks that he accuses the profession of robbing their clients. He states that in London it is a known fact that grooms expect a percentage off the accounts of tradesmen. Mr. Banham is not wide of the mark if he applied the term to some of us, as we are styled and known by the title of shoeing smiths, etc. He states he is of opinion that there are few practitioners who have not been asked what percentage they give. For Mr. Fraser's information, I may state I have, and by London coachmen.

Mr. Banham has not accused us of consenting to this arrangement; he points out that if such did take place what the effect would be, and that we could not afford to be the losers. I for one do not object to give a groom, coachman, or herdsman an honorarium when he has served me well in a case; but what I do object to is the increasing Christmas-box system —another mode of "tipping"—and I can say I have lost more than one good client in refusing to give a sum that I considered (as Mr. Banham puts it) "a robbery." I do not quite understand Mr. Fraser's protesting against Mr. Banham's remarks, when he concludes by saying so long as it exists (meaning the custom) in this country we must bow to it.

A. H. SANTY, F.R.C.V.S.

AMPUTATING HORSES' TAILS.

SIR,—*Is it illegal* to dock horses and other animals? That is the question. If it is, the fact remains vague and misty in the minds of the profession and the public. Therefore on behalf of the former I beg to petition the President of the Royal College of Veterinary Surgeons, to allow the College solicitor to look up this matter, with a view to your kindly publishing the particulars in an early issue of the Journal, requesting that gentleman to cite for the purpose any Act of Parliament or other order bearing on the question, and to furnish copy of the clause or clauses declaring the practice *to be illegal*, together with the forfeits or penalties for infringement; also to state whether the operator is declared to be the sole culprit, or the owner of the animal is considered to be *particeps criminis*. Veterinary surgeons will then be in a position to explain to their clients that refusal to comply with the requisition is in obedience to the law. If it be illegal to dock animals, then certainly this law, "is more honoured in the breach than the observance," to an extent almost without parallel. Since the controversy, some time back, in your Journal and others, the practice has in no way diminished. It would

seem even that more horses are docked now than was the case a few years ago, as, with the revival of the fashion for short tails, many horses are now being docked more than once. Probably at the present time the only individuals who refuse to operate are to be found amongst the veterinary profession, and these the very persons who would, if need be, perform the task with the least degree of pain to the animal. The refusal of the veterinary surgeon in no way minimizes the instances of docking, for if it be the owner's fancy that it should be done, done it is, by some one else directly.

October 8th, 1886. "LEX."

[As this matter stands at present, there have been, we learn, as many convictions for docking horses as there have been dismissals. So that there are only magistrates' opinions to go by, and these are divided as to the uselessness of the operation, and therefore as to its cruelty. Until a judge's decision has been obtained the subject will remain in dispute. There can be no doubt as to the wide prevalence of the fashion of chopping off horses' tails, and chiefly those of polo ponies and saddle-horses, to which the harness plea cannot apply. In the great majority of these, the mutilation has been carried to a disgusting extent, only a few inches of the appendage being left. The fashion is certainly a cruel and a degrading one, and the outcome of a morbid and vicious fancy.]

THE HORSE AND ITS TAIL.

SIR,—As I have been frequently spoken to of late on the above subject, and because confused and erroneous ideas are still held by many people regarding the docking of horses, I hope you may deem it of sufficient importance to accord me space in your valuable paper to merely enumerate the principal arguments for and against this practice, which I shall endeavour to do in as short a manner as I possibly can, hoping that it may enable those who are not quite decided on the matter to judge for themselves whether it is necessary or not, and assist them in arriving at a proper conclusion. I would plead for this privilege on behalf of our noble friend the horse. The most ardent supporters of the custom advocate it now only in certain instances, that of hunters, hacks, and harness horses, but maintain that it is necessary in those, the reasons being:

1. The tail is too long, especially of some horses.
2. It prevents it getting over the reins when driving.
3. It prevents it coming in contact with the dashboard.
4. It improves his appearance.
5. It increases his value.
6. It is customary.
7. It conduces to cleanliness and adds to the comfort of the rider.

The reasons why it should not be done are:

1. Because the tail is the proper length best suited to its requirements. All horses' tails are the same length, the same number of bones being in each, and are in proportion to the length of the body, with some rare exceptions.
2. It does not prevent it getting over the reins; the proper remedy being to raise the rest for them, and the higher the better.
3. It does not prevent it coming in contact with the dashboard. To do this lengthen the traces and shafts.
4. It does not add to his beauty, only gratifies a morbid taste.
5. It does not increase his actual value; only imaginary.
6. It is unnecessary, and custom is no justification.
7. Removal of the long hair is all that is necessary for domestic purposes and comfort.

8. It does not benefit the animal itself.
9. It is dangerous to the life of the animal.
10. It is cruel; giving rise to pain and suffering.
11. It deprives him of his natural protection against flies and insects.
12. It destroys the flexibility of the tail, and increases the difficulty of extricating the reins.
13. It lessens his power of turning and balancing the body.
14. It affords protection and covering to delicate and tender parts.
15. It limits his sale; docked horses not being allowed for army purposes.
16. Race-horses, and horses in America, Germany, and other countries are not docked.
17. It is not a surgical nor scientific operation.
18. Other like practices have been abandoned; such as nicking and ear-cropping.
19. It is not so common now as formerly, and ought to be entirely abolished.
20. It is unnatural and unreasonable.
21. It is a barbarous practice, and repugnant to the best feelings of humanity.

If it should become necessary to deprive an animal of part of its tail proper, either from accident, disease, or malformation (these being the only reasons, in my opinion, which can justify the privation), it ought to be removed in a proper and surgical manner, and afterwards treated like any ordinary wound, and not as it is commonly done—a piece chopped off any how, the parts being afterwards roasted with a hot iron. All that is required to put a stop to this practice is that the ladies and a few hunting gentlemen should take the matter up, and it will soon be done for. Thanking you in anticipation,

I am, yours faithfully,

JAS. P. S. WALKER, F.R.C.V.S.

Oxford, 22*nd Sept.*, 1886.

DUPLICATE COMMUNICATIONS.

DEAR SIR,—I am in a quandary. For years I have taken the *Veterinarian*, *i.e.*, January, 1855, and the Journal from its first issue. Of late I have found the subjects and matter in each issue almost identical, and to such an extent that it seems absurd to take both journals.

Firstly, I decided to drop one, but on trying to decide which it should be, I determined on second thought to write you, sir, as the *junior*, if it is not a general rule in journalism for authors of articles—indeed all writers—to stipulate that each and every article shall be sent to that one journal only?

If this rule was observed it would prove decidedly beneficial to each journal, by increasing the circulation of them, for surely there are many members at present who do not take both because of the sameness of material.

I shall be sorry to discontinue either, but certainly shall, rather than take one, the *ghost* of t'other.

U.S.A. M.D.

[We have more than once notified to our contributors the rule we laid down at the commencement of the VETERINARY JOURNAL, that we could not undertake to publish, as "original communications," papers copies of which had been sent to other periodicals. Unfortunately, this rule has escaped the notice of writers at times, and this oversight we could not discover until too late. We can only once more direct attention to the rule, with the request that it may be observed.]

THE LATE PROFESSOR DICK.

DEAR SIR,—It might be interesting to many of your readers, especially those who were formerly students with the late Professor Dick, to know that they could obtain an excellent cabinet portrait of him at the undermentioned address. When a student in Edinburgh, I obtained one of his *carte-de-visite* portraits, and lately asked a friend of mine if he could enlarge it; this he has done, and I am so well satisfied with the result, that I should like the profession to know where they may be obtained. The address is Mr. J. White, Photographer, 32, High Street, Littlehampton, and the price is two shillings.

"AN ADMIRER OF PROFESSOR DICK."

C. CUNNINGHAM, Slateford, writes to ask for insertion of the following emendation in his paper on "Inoculation in Pleuro-pneumonia," which appeared in our last month's issue. In the last paragraph but one is a sentence, "Then let inoculation be made compulsory." As this may convey an impression different to that which he intended, he wishes the sentence to stand thus: "Then let inoculation in every infected place, or area, be made compulsory," his opinion being that all the cattle in an infected place, or in direct contact with diseased subjects, should be inoculated compulsorily, and not that it should be obligatory all over the country, among healthy and infected stock alike.

INQUIRER has a wide choice of books on agricultural chemistry. "The Chemistry of the Farm," a little cheap manual, just published by Bradbury and Evans, is good as a popular book.

THE next meeting of the Royal Counties Veterinary Medical Association will be held at the Great Western Hotel, Didcot, on November 26th, when Mr. Wilson, Berkhampstead, will read a paper on "Anthrax." That of the Liverpool Association will take place in the Medical Institute, Hope Street, on Nov. 12th, at 6 p.m.

Communications, Books, Journals, etc., Received.

COMMUNICATIONS have been received from A. H. Santy, Norwich; F. Raymond, A.V.D., Woolwich; "Lex"; K. J. Urquhart, Norwich; C. Hartley, Lincoln; R. H. Dyer, Limerick; R. W. Burke, Cawnpore; J. E. Adlard, London; "M.D."; E. A. Hollingham, Tunbridge Wells; W. O. Williams, Edinburgh; A. Gresswell, Louth; J. B. Gresswell, Louth; G. A. Banham, Cambridge; C. Cunningham, Slateford; "Inquirer"; A. Broad, London; J. P. Walker, Oxford; P. M. G. Williams, Haverfordwest; J. W. Ingram, Manchester; "Honestas"; F. Smith, A.V.D., Aldershot; "An Admirer of Professor Dick"; H. Kidd, Hungerford; T. Chambers, Wolverhampton; W. E. Litt, Shrewsbury; Professor McCall, Glasgow; "Peter Ramus"; W. Penhale, Devon; A. Marking, Sudbury.

BOOKS AND PAMPHLETS: *A., J. B. A., and G. Gresswell*, Diseases and Disorders of the Horse; *E. Ellenberger u. J. Schütz*, Jahresbericht über die Leistungen auf dem Gebeite der Veterinär-Medicin; *A. Hoffa*, Die Natur des Milzbrandgiftes; *E. Imlin*, Viehsouchenbulletin von Elsass-Lothringen.

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